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A paper presented at the 162nd Meeting of the American Institute of Electrical Engineers at New York, February 28th, 1902.

THE ELECTROCHEMICAL INDUSTRIES.

BY SAMUEL SHELDON.

Introduction.—In this paper an attempt is made to give a general view of the present condition of the electrochemical industries. The subject of storage and primary batteries is so large and is so clearly differentiated from the other electrochemical industries that it is not touched upon. Neither is the subject of electrotyping considered. The carrying on of the other industries requires over 200,000 horse-power. Of this about 90 per cent. is obtained from water power. The cost of electrical power, obtained from water power, varies much in different parts of the world. Swan gives the limits per horse-power year as $5 and $50. At Niagara the price to large consumers is about $17. This power is delivered in Buffalo for about $30.

Electro-Deposition of Metals.

Electro-Plating.—The methods of plating employed in establishments in the vicinity of New York do not differ to any extent from the methods well-known in the art. The average practice may be described as follows:

Gold.—There are three solutions in common use to give the three colors of deposit known as "California gold," "green gold," and "red gold." The salts to form the solutions are prepared by dissolving, in nitro-hydrochloric acid, the following alloys: For California gold, an alloy of 22 parts gold and 2 parts silver; for green gold, an alloy of 16 parts gold and 8 parts silver; and for red gold, an alloy of 16 parts gold and 8 parts copper. Upon evaporation the chlorides of these metals

remain. They are dissolved in a solution of potassium cyanide to form the plating solution. Double cyanides are thus formed. A freshly prepared solution contains a pennyweight of gold to the gallon. The anodes are of pure gold. The articles to be gilded form the cathodes. The rate of solution of the anodes is not as great as the rate of deposition on the cathode. Therefore the solution has to be enriched by frequent addition of the salts. The current density is adjusted by varying the depth of submergence of the anode, the baths being supplied with five volts at constant potential. The correctness of the current density which is being employed is determined by the eye and the experience of the attendant. There is no circulation of the electrolyte.

Silver.—The solution consists of a double cyanide of potassium and silver, the concentration being two ounces of silver per gallon of solution. The anodes are of pure silver. Owing to the tendency of silver to deposit in arborescent crystals, the articles which are being plated are moved backward and forward in the bath in a plane parallel with the anode surface. The path of movement is about two inches and the time required to traverse it about three seconds. The proper current density rests with the judgment of the attendant. Too great a density or an impoverished solution is indicated by a violet tinge imparted to the deposit. Silver-plated articles to have a polished finish require burnishing with steel or blood-stone burnishers moistened with a solution of soap and distilled water.

Nickel.—The solution universally employed is one of nickel-ammonium sulphate. The proper concentration results from dissolving eight pounds of the salt in a gallon of water. The solution should be kept neutral by the addition of either ammonia or sulphuric acid. The proper current density is from 0.5 to 1.5 amperes per square decimetre of cathode surface. Nitric acid should not be present in any form in the solution.

Brass.—Much house or builders hardware consists of brass-plated steel or iron. Many solutions are used. The best, perhaps, contains equal quantities of zinc and copper cyanides or carbonates dissolved in ammonium carbonate. The color of the deposit may be varied by changing the relative amounts of the copper and zinc salts.

Critical Current Density.—In the electro-deposition of metals from a salt solution the rate of deposition, which may be used, is limited. If a proper current density at the cathode be em-

ployed, an adherent reguline deposit will result. If the current density be increased, a limit is reached, when, if exceeded, the character of the deposit will change, becoming pulverulent and non-adherent. This limit is termed the *critical current density*. As an increase of the permissible current density allows of a smaller plant for the same output and shortens the time that the metal in the solution is idle, a consideration of the factors which determine the value of the critical current density is pertinent. Consider a cubic centimetre of metallic salt solution at rest, and directly in front of a cathode. It contains a certain weight of the metal. This weight is dependent upon the concentration of the solution and can be calculated from its specific gravity. A certain percentage of this metal, dependent on the temperature and concentration of the solution, is dissociated and takes part in the electrolytic conduction. If an attempt be made so to adjust the current flowing through this cubic centimetre, that by Faraday's laws the rate of deposition is such that all the dissociated metal ions must be deposited in one second, then, it is evident, the velocity of movement, or migration of the ions, would be one centimetre per second. Such a rate of deposition of ordinary metals from a stationary solution is impossible. The maximum velocity of migration is rather to be reckoned in thousandths of a centimetre per second. If the deposit is to remain reguline, the ions must not be expected to migrate faster. It is possible, however, to adjust the circuit for any current density. As soon as the rate of deposit exceeds the ability of the metal ions to supply it, hydrogen of the solvent appears to take part in the conduction and a pulverulent deposit results, which contain some hydrogen and is said by some to be a hydride of the metal. The critical current density may be increased by increasing the concentration of the solution, by raising its temperature, or by *circulating the electrolyte toward the cathode*.

Circulation.—Circulation of the electrolyte has three functions. It permits of a higher rate of deposition, as mentioned above; it replenishes the part of the electrolyte next to the cathode which has been deprived of its metal; and it prevents, to a certain extent, the deposit from assuming a nodular or aborescent form. When deposition is taking place at moderately large current densities, accidental differences of concentration occur in the vicinity of the cathode. Places on the cathode which can be reached by current flowing through the more concentrated portions of the electrolyte will receive a heavier deposit than

other places. As a result they will protrude slightly from the cathode. These unevennesses are multiplied and magnified as the deposition continues.

Dissociants.—The water of an ordinary electrolyte performs two duties. First, it acts as a solvent, whereby it allows and facilitates the uniform distribution of the dissolved substance throughout the solution, subdivision of the substance being carried to a condition of a molecular fineness. Secondly, it acts as a dissociant whereby some of the molecules of the dissolved substance are split up and dissociated into their component ions. Without dissociation there can be no electrolytic conduction of the current. Besides water the following substances are common solvents and dissociants: liquid ammonia, ethyl alcohol, methyl alcohol, glacial acetic acid, and pyridin. The last-named contains no oxygen, and it has been recently shown by Dr. Kahlenberg that from a solution of lithium chloride in it, pure metallic lithium may be electrolytically deposited.

Copper Refining.—Refined copper constitutes the most valuable product of all the electrochemical industries. The processes of producing the refined copper from its alloys have been well described by Badt in the ninth volume of the INSTITUTE'S published TRANSACTIONS. The copper alloy, or blister copper, is cast into slabs which are used as anodes in a bath containing a slightly acidulated solution of copper sulphate. This electrolyte is kept in continuous circulation. Pure copper is deposited from the solution upon pure copper cathodes at a current density of from 15 to 25 amperes per square foot. Copper is simultaneously dissolved at the anode, and the alloyed gold, silver, and other metals drop, as slime, to the bottom of the cell to be parted and recovered later. After deposition the cathodes are melted up, poled, and cast into wire-bars or other suitable forms. The " poling" of copper consists in stirring the molten metal with a pine stick. The stick carbonizes, and, as a result, reduces cuprous oxide which is present in the molten copper. Upon cooling, cuprous oxide gives up its oxygen to metallic impurities which may be present in the copper. As the mechanical and electrical properties of copper are better when impurities exist in the form of oxides rather than as metals, the proper amount of poling is when just sufficient cuprous oxide is left to oxidize the impurities. Overpoled or underpoled copper is brittle and of inferior conductivity.

Parting of Gold and Silver.—This is accomplished by means

of electrolysis very satisfactorily in the following manner. The alloy of the two metals constitutes the anode, and is placed in a linen bag. The cathode is a thin sheet of pure silver. The electrolyte is at first a one-tenth per cent. solution of nitric acid, which ultimately becomes a dilute solution of silver nitrate. With a current density of 33 amperes per square foot, silver 999.5 per mil fine is deposited on the cathode in the form of beautiful crystals. These are mechanically scraped off and allowed to drop to the bottom of the cell. Gold, in the form of mud, containing some silver, collects in the anode bags. The silver is removed from it by dissolving in nitric acid, leaving the gold in a very pure condition.

Extraction of Gold.—The process of Siemens and Halske, which is in use in South Africa, consists in dissolving the gold from the crushed ore in a solution of cyanide of potash and then subjecting the solution to electrolysis with iron anodes and thin sheet lead cathodes. The anodes are dissolved. Dr. Keith improves upon this process by adding a small amount of soluble mercury salt to the cyanide solution. It increases the rapidity of solution, and mercury is deposited alongside the gold on amalgamated copper cathode plates. The amalgam is scraped from these plates and the gold and mercury separated by "retorting."

Extraction of Nickel.—By the Hoepfner process the nickel ores are first roasted, crushed, and leached in a mixed solution of cuprous chloride and calcium chloride. By electrolysis with carbon electrodes the copper is removed from the solution. The lead and iron are removed by chemical means. There then remains a solution of nickel chloride. This is electrolyzed, and the nickel deposited upon a nickel cathode.

Inorganic Products.

Alkalies and Bleach.—The power employed in the electrolytic preparation of these substances amounts to more than 50,000 horse-power. There are many different processes employed, some of which are secret. The electrolyte is always common salt, either in water solution or in a fused condition. Upon passage of a current, using a carbon anode, chlorine is developed in the gaseous form at the anode and is led to chambers containing lime with which it forms bleaching powder. Metallic sodium appears at the cathode. If the cathode be mercury, as in the Castner process, a sodium amalgam is formed, which, upon wash-

ing with water, yields caustic soda and returns the mercury. If the cathode be iron in a water solution, caustic soda is developed by secondary reaction. In the Acker process the electrolyte is fused salt and the cathode is molten lead.

Sodium.—This is manufactured on a commercial scale at Niagara by the electrolysis of fused caustic soda. The electrolyte is maintained in a fused condition by the current which passes through it. An iron containing vessel constitutes the cathode, and the anode consists of rods which dip into the electrolyte. The sodium after deposition rises to the top of the electrolyte, and floats upon its surface. As it accumulates it is removed by small iron dippers. The sodium, after its reduction, is used in the manufacture of sodium dioxide. This is done by spreading the metallic sodium on trays, which are placed in tubes, supplied with air and dried over calcium chloride. The sodium peroxide is then used for producing peroxide of hydrogen. For this purpose it is mixed with sulphuric acid, and during the mixing the temperature is maintained near zero by melting ice. This is necessary because of the unstable character of the peroxide of hydrogen.

Aluminium.—This metal is reduced electrically from alumina by the Pittsburg Reduction Co. at Niagara Falls in America, and at Shawinigan Falls, in Canada, and by six other concerns in Europe. The annual product in America is about 3,000 tons. Kershaw estimates the total output for 1901 as 7,500 tons. The double fluoride of aluminium and calcium, in a fused state, is used as a solvent and dissociant. The heat developed by the passage of the electrolyzing current maintains the solvent in a fused state. The electrolyte is contained in large cavities made in carbon slabs. The carbon constitutes the cathode, and group of large carbon rods, dipping into the fused electroyte, constitute the anodes. The metal is drawn off once a day through taps leading to the bottom of the cavities. As the electrolyte is deprived of its metal its resistance rises. This fact is utilized to give warning to attendants. An incandescent lamp is shunted between the electrodes and, as the resistance of the electrolyte rises, the brilliancy with which the lamp burns increases. The attendant then introduces fresh alumina to the fused electrolyte. The process is thus continuous and uninterrupted.

Sponge Lead.—This is produced from litharge by placing it in contact with a sheet lead cathode in an electrolytic cell having a lead anode and dilute sulphuric acid as the electrolyte. The

deposited hydrogen reduces the litharge, leaving a lead sponge of fine quality. This process is employed by the National Battery Co. at Buffalo. The Electric Lead Reduction Co. of Niagara Falls is also producing sponge lead by subjecting galena to electrolysis.

Potassium Chlorate.—This is manufactured in considerable quantities by the National Electrolytic Co. at Niagara Falls by the electrolysis of a solution of potassium chloride. The process is not ideal as compared with many other electrolytic processes and the yield is not as great as could be desired for the electrical energy which is employed. The yield is increased by introducing a small quantity of potassium chromate into the electrolyte, as shown by Muller, and the introduction of a little alumina neutralizes the potassium hydrate as it forms at the cathode. The cathodes, as used at Niagara, are of wire gauze covered with cuprous or cupric oxide. The reduction of the oxides by the liberated hydrogen develops sufficient heat to maintain the temperature of the electrolyte at 50°C. The electrolyzed solution is led into refrigerating tanks, where the chlorate is made to crystallize by lowering its temperature.

Ozone.—This gas is a modified form of oxygen. It is generally stated that a molecule of ozone contains three atoms of oxygen while a molecule of gaseous oxygen contains but two atoms. This, however, is uncertain. Ozone differs from ordinary oxygen in being a much more powerful oxidizer at ordinary temperatures. It very quickly oxidizes organic substances. Ordinary rubber becomes brittle and crumbles after an immersion for a short time in the gas.

There are many forms of apparatus devised for the electrolytic production of ozone. The general principle underlying the construction of all of them is to pass a stream of dry oxygen or air between two conducting surfaces which are close to each other. The conductors are subjected to differences of potential which are rapidly varied in amount over wide limits. The high tensions of electrostatic machines, induction coils, or transformers are used. Disruptive discharges may be employed for producing the rapid change of potential difference. One company is using an alternator which gives 10,000 cycles per second. The issuing gas is said to be ozonized. At best it contains but little above one per cent. of ozone. A molecule of oxygen, consisting of two atoms, each bearing an electric charge, when subjected to the sudden variations in the electrostatic field between the con-

ductors, may in some or many cases be torn apart. The constituent parts may join with other molecules of oxygen to form ozone molecules.

Ozone is used on a commercial scale in yielding organic oxidation products, in sterilizing water for potable purposes, and in bleaching fabrics. Among its proposed uses may be mentioned the decoloration and defecation of syrup in sugar manufacturies, the aging of wines, and the aging of woods for the construction of musical instruments such as violins.

Nitric Acid.—One firm which is interested in electrochemistry, has perfected a method for the electrolytic production of nitric acid, the nitrogen being recovered from the atmosphere. The electrical process very materially reduces the cost of production. The importance of such a process may be very great in the future. Nitrogen is essential in the diet of all the higher forms of animal life. Soils are deprived of their nitrates by the crops which are grown. The supply is replenished to some extent by parasites which grow in nodules on the roots of leguminous plants. These parasites are capable of taking nitrogen from the atmosphere and oxidizing it. And yet fertilizers containing nitrates have to be applied to the soils to maintain their productiveness.

Organic Products.

Several hundred organic substances have been prepared in the laboratory by means of electrolysis. Most of them remain as laboratory preparations, but some have come upon the market, being manufactured on a commercial scale. All these organic products may be arranged in four classes, depending upon the ultimate constitution of the substance yielded as compared with the composition previous to electrolysis. Without considering the actual electrolytic process, the effective process is either one of oxidation, of reduction, of concentration, or of substitution or addition. These processes, like all electrical processes, are especially valuable because of the extreme delicacy of regulation which is possible. The efficiency of the process as regards yield is thereby increased.

Electrolytic methods constitute a valuable addition to the facilities at the disposal of the organic chemist. In carrying on experimental work attention should always be given to the temperature, to the concentration, and to the current density which is being employed.

Iodoform.—This is prepared by the electrolysis of ethyl alcohol in a solution of sodium or potassium iodide. The anode is of platinum and is placed in a solution made of 15 parts of potassium iodide, 100 parts water, and 16 parts of ethyl alcohol. This solution is separated by a porous diaphragm from a strong solution of sodium hydroxide, which contains a nickel cathode. The temperature of the electrolyte is kept at 70°C and a current density of one ampere per square decimetre is employed. After passage of the current for three or four hours, the solution is allowed to cool and iodoform crystallizes out.

Chloroform and Bromoform.—It is claimed that by substituting chlorides or bromides for the iodides in the above process either of these two substances may be prepared.

Camphor.—If turpentine hydrochloride, in a melted condition or in an alcoholic acetic acid solution, be electrolyzed, camphor is obtained. Camphor results from the oxidation of terbene, *i. e.*, oil of turpentine. Artificial camphor is made from turpentine in large quantities by the Ampere Electro-Chemical Co., but the process is claimed to be in nowise electrical.

Vanilin.—This substance, which is the odoriferous component of vanila, is obtained by the oxidation of eugenol (oil of cloves). It is much used in the manufacture of perfumes. It is produced electrically on a commercial scale in France.

Artificial Rubber.—A process for the electrical production of artificial rubber is being experimented with, and it is said to promise much for the future. No details concerning the method are being given to the public.

Furnace Products.

The electric furnace owes its great utility to the facility with which its temperature can be regulated and especially to the great elevation of temperature which is obtainable. The amount of heat which is produced in a conductor is dependent upon the resistance of the conductor, the strength of the current, and the duration of the current flow. All of these are easily altered in magnitude. The temperature of the conductor is determined by the rate of development of heat and upon the facility which is offered for its escape. The ultimate stationary temperature is attained, when the rate of escape of heat is equal to the rate of its production. With a given rate of heat production the temperature rises as the facility for the escape of heat is lessened. The many available heat insulators makes it possible to produce

any temperature which can be withstood by the conductors.

Calcium Carbide.—This substance, which is prepared by heating a mixture of coke and lime in an electric furnace, constitutes at present the most valuable electrochemical product with the exception of copper. Furthermore, its production requires more power than any other electrochemical industry. Kershaw estimates that in 1900 the power employed was 85,000 horse-power. The rate of production has been in excess of the rate of consumption and the result has been a decrease in the price of the product.

Its value lies in the fact that, when placed in water, acetylene gas is developed to the extent of about five cubic feet per pound of carbide. When burned in air the acetylene gives a flame of great intrinsic brilliancy and of pleasing color quality. Its richness in carbon tends toward a sooty flame. Swan inclines to the belief that the gas may prove of great value in metallurgy as a reducing and carbon carrying agent.

Calcium carbide is put in the class with explosives and dangerous chemicals. It is therefore subject to high freight rates for transportation and is associated with high insurance rates. Owing to the moisture ever present in the atmosphere, it suffers from freight transportation over long distances. These facts militate somewhat against its adoption as a source of illumination for small towns.

Carborundum.—This substance is carbide of silicon. It is an abradent, harder than emery, and is used for the same purposes as the latter. It is made into wheels, into tools of various forms, and into hones. The process of manufacture consists in heating electrically to a very high temperature, a mixture consisting of 60 per cent. pure sand and 40 per cent. powdered coke. To this mixture is added a small amount of common salt and sufficient sawdust to render the mass porous, so that gases, which are liberated in large quantities, may escape. This mixture is packed around a "core" of coarse granulated coke, the grains having diameters of from $\frac{1}{4}$ inch to $\frac{3}{8}$ inch. An alternating current is sent through the core and produces heat, which does not easily escape through the surrounding mixture. The result is a great temperature elevation and the formation of the carbide, some of it being in the form of very beautiful violet-colored crystals.

Artificial Graphite.—Upon the common introduction of arc lighting it was noticed by many observers that the tips of remnant carbons were transformed into something resembling

graphite. The transformation extended only over portions of the tips which had been heated in the lamp to a high temperature. In the manufacture of carborundum it was observed that there was always produced a layer of graphite in the hottest part of the furnace. Upon investigation the conclusion was reached that graphite was not formed by merely heating pure carbon to a high temperature, but that a carbide must be first formed and afterwards be decomposed by the electric furnace heat. Therefore it is necessary to have some metallic salt or oxide mixed with the carbon before heating. In practice a good mixture consists of 97 parts of finely divided amorphous carbon commingled with three parts of iron oxide. This mixture permits of moulding into any desired shape, which is afterwards graphitized. The moulded articles are placed inside of a hollow carbon cylinder of about 20 feet in length and 20 inches in diameter. Current is sent through the cylinder in a longitudinal direction. As the articles become graphitized they acquire a greater conductivity, and the current which flows under a given impressed pressed voltage increases accordingly. In practice the original impressed voltage is about 150 and the amperage about 300. As the graphitization proceeds, the voltage is decreased, although the amperage increases.

Barium Hydrate.—The United Barium Company is manufacturing about 60 tons per day of this substance at Niagara. It is in the form of large crystals of an inch or more length on the edge. The manufacture by the electrical method has reduced the price of the product to one-quarter what it was previously. It is consumed in large quantities in the manufacture of pigments, in the purification of water, and in the recovery of sugar from beet molasses. In the last case a ton of hydrate is consumed in the recovery of a ton of sugar. The hydrate is made from crude barytes, this being the only natural source of barium. One equivalent of barytes is mixed with three of barium sulphate, and the mixture is heated in an electric furnace. SO_2, is driven off in the form of vapor, leaving barium oxide behind. This oxide, upon being placed in water, becomes hydrated. It is then allowed to crystallize.

Barium Cyanide.—This substance is made by the Cyanide Company at Niagara. It is used in the production of the other cyanides. Barium carbide, a substance which is the analogue of calcium carbide, is mixed with coke, and producer gas is passed through the mixture while it is being heated electrically. The

carbide has an unsaturated molecule, and the three free bonds of each carbon atom easily take an atom of nitrogen.

Phosphorus.—This is manufactured by the Oldbury Electro-Chemical Co. The electrical process is practically the same as the chemical process, except as to the method of applying heat. Calcium phosphate, either in the form of bones or of natural rock phosphate, is pulverized, mixed with coke, and placed in an electric furnace. Upon passing current through the mixture, the phosphorus becomes vaporized and is collected under water. The electrical process is cheaper than the older chemical one. This is because of two reasons. The temperature necessary to vaporize the phosphorus is quite high. There is therefore a large loss of heat when it is applied to the outside of the closed retorts containing the phosphate. This loss is not present when the heat is produced electrically inside of the retort. Again the practice of the older process was attended with much breakage of retorts, which is absent in the case of the newer.

Corundum.—The Norton Emery Wheel Co. is manufacturing five tons per day of this artificial emery, which differs from the natural emery in being absolutely pure and of uniform specific gravity. The natural variety lacks these two qualities which are essential in the manufacture of a perfectly satisfactory emery wheel. The process of production consists simply in the heating of bauxite in the electrical furnace until water is driven off and the remaining alumina becomes molten. Upon cooling, this latter assumes a crystalline form.

Iron and Steel.—There are a number of experimental plants in various parts of the world manufacturing steel directly from iron ores by means of the electric furnace. None of them has, however, passed the experimental stage. A number of alloys of iron, which are found useful and valuable in the manufacture of steel, are made in considerable quantities by means of the electric furnace. Among these may be mentioned ferro-chromium, ferro-titanium, and ferro-silicon. The first was used in the manufacture of armor plates intended for the cruiser made in this country for the Russian Government.

Silicides.—A very interesting line of substances has been produced by the Ampere Electro-Chemical Co., by means of the electric furnace, which it has termed *silicides*. They are the analogues of the carbides. Especially interesting is silicide of barium, which decomposes when placed in water with a development of hydrogen gas.

MISCELLANEOUS.

Electrolytic Condensers.—If two carbon electrodes in a solution of zinc sulphate have impressed upon them a small difference of potential, a current will flow until the counter electromotive force of polarization, resulting from the deposition of zinc on the cathode, is equal to and opposes the impressed e. m. f. Upon removing this latter e. m. f. without opening the circuit, an inverse current will flow until the zinc has been redissolved. Such a combination constitutes an electrolytic condenser, its charge being the quantity of electricity associated with the deposited zinc. Its capacity is this quantity divided by the polarization e. m. f. produced by it. This capacity is very large and increases with the area of the submerged portions of the cathode. Such condensers cannot be utilized with potential differences in excess of the maximum polarization e. m. f. They are also very inefficient owing to leakage currents necessary to compensate for local action. For use with higher voltages cells may be used in series. For the same volt-ampere capacity, electrolytic condensers occupy about the same space as ordinary condensers of paraffined paper and tin-foil. They have all the disadvantages associated with liquids.

The Electrolytic Rectifier.—This device in its latest form, as designed by Pollak, consists of an electrolytic cell containing a slightly acid solution of potassium phosphate as an electrolyte. One electrode is of plain lead and the other is of aluminium, which has been formed by macerating it for some time in a solution of caustic soda. If the pressure on a circuit, in which this cell is connected, does not exceed 200 volts, and the temperature of the electrolyte be under 40°C., then the cell will allow current to pass through it from lead to aluminium, but not in a reverse direction. This action is due to the formation of an insulating skin of aluminium hydrate over the surface of the aluminium when it is functionating as an anode. By means of such a cell one lobe of an alternating current cycle can be extinguished. The life of a formed aluminium plate is about 500 hours. The skin then breaks down and separates from the plate.

Electrolytic Interrupters.—The Wehnelt interrupter consists of an electrolytic cell with dilute sulphuric acid as an electrolyte, with a plain sheet lead cathode and with an anode of platinum wire, exposing a small surface (say $\frac{1}{4}$ cm^2). The Caldwell interrupter consists of a cell, with almost any electrolyte, with both electrodes of carbon, lead, or other suitable material, the cathode

with its surrounding electrolyte being separated from the anode and its surrounding electrolyte by an insulating diaphragm containing one or more minute perforations. Upon introducing either of these interrupters into a circuit a series of sudden interruptions of the current will occur. These are due to the evolution of non-conducting gases around the platinum wire in the one case and in the diaphragm perforations in the other case. After interruption the circuit closes again as soon as the gases arise, due to their buoyancy. The frequency of interruption can be easily regulated. It increases with increase of impressed voltage, with decrease of inductance and resistance in the circuit, with decrease of exposed anode surface, and with increase of hydrostatic pressure at the place of interrupting gas development. When properly designed and used in the primary circuits of induction coils, these interrupters produce remarkable results. The suddenness with which the magnetic flux which is linked with the secondary, can be made to disappear is responsible for the high e. m. f. developed. The great amount of energy localized at the point of interruption of the circuit is destructive to the surrounding insulation. Lava is employed with considerable success.

POLYTECHNIC INSTITUTE OF BROOKLYN,
Feb., 1902.

A paper presented at the 162d Meeting of the American Institute of Electrical Engineers at New York, February 28th, 1902.

THE ELECTRIC FURNACE IN INDUSTRIAL CHEMISTRY.

BY CHARLES B. JACOBS.

In its early days the electric furnace labored under the burden of the prophecies of its wonderful possibilities and products, and seems to have been more or less associated in the popular mind with the production of diamonds from amorphous carbon, the formation of gems and precious stones from cheap and plentiful materials, and other somewhat sensational operations which had a tendency to obscure, for a time, the knowledge of its usefulness in the field of industrial chemistry.

Notwithstanding the fact that DeChalmot, the Cowles brothers and others, had been operating the electric furnace for some years in the production of alloys from very refractory ores, it was not until Acheson produced carbide of silicon, Willson obtained carbide of calcium in commercial quantities, and Moissan published his scientific investigations in *Le Four Electrique*, that the value of the electric furnace for the production of substances useful in the arts began to be generally recognized.

Since that time, however, its field of usefulness has been steadily increasing until it is now obtaining recognition as an important factor in industrial chemistry.

Reference to the patent lists of the various countries will show an immense variety of electric furnaces, but they may all be classified under three general types which have been modified in numerous ways to meet the special requirements of the various processes for which they are intended.

The three general types of the electric furnace are:

(1) The reflecting are furnace of the Moissan type, in which

the heat of the arc is projected upon the charge by a reflecting surface.

(2) The direct-heating arc furnace, in which the arc is surrounded by the material treated and imparts its heat directly to the charge.

(3) The incandescent furnace, in which either the charge or the walls of the furnace form a part of the electrical circuit and by the resistance offered to the passage of the current produce the required heating effects.

The direct-heating arc and the incandescent furnace are the types most frequently met with in commercial work.

The electric furnace, of whatever type, is, properly speaking, nothing more than a machine capable of producing a high degree of thermal efficiency, for it is not usual in electric furnace work to depend on the current for any electrolytic action; in fact, the alternating current is more regulable and seems to give better heating effects than direct current. When electrolytic action does come into play the furnace reverts to the electrolytic cell of the type in which fused substances are electrolyzed, but even here the effects depend solely on thermic conditions. The compounds formed or decomposed by a given expenditure of electrical energy are directly dependent upon and calculable from the heats of formation of the compounds in question, by virtue of the relationship which exists between the voltage, heat of formation of chemical compounds and their electro-chemical equivalents.

The true type of electric furnace is purely thermal in its effects and has no concern with electrolytic action.

The quantity of heat which may be liberated per second in an electric furnace varies from a few hundred calories in small laboratory furnaces to many thousand calories in large furnaces of commercial capacity. The intensity of the heat or the temperature which may be obtained in an electric furnace is limited and controlled by various conditions, chief among which are the refractability and heat conductivity, or rather lack of heat conductivity, in the material of which the furnace is constructed.

In furnaces constructed with linings of pure carbon blocks reinforced with some very poor conductor of heat, such as magnesia or chalk blocks, it is possible to obtain a temperature close to 4000 C., but at this temperature the purest carbon begins to vaporize.

The temperature to which the charge in the furnace may be

raised is of course dependent upon the weight and specific heat of the material to be treated, and this data being known, the temperature obtainable may be calculated with some degree of exactness, especially when the duration of the heating is short and the charge is enclosed in furnace walls of very low heat conductivity, so that the heat generated is mainly absorbed by the charge.

It is comparatively easy to operate commercially at temperatures ranging from 2500° to 3500° C. and to raise the charge almost immediately to the required temperature. Operating at these high temperatures, which may be constantly maintained until the desired operation is completed, reactions will take place between elements which have apparently no affinity for each other at the temperatures obtainable in ordinary combustion furnaces.

All are more or less familiar with the results obtained by Moissan in his electric furnaces, and Borchers claims to have gone far toward proving that every known oxide may be reduced by carbon in the electric furnace.

Among some of the most striking of the new chemical compounds of the electric furnace are carbide of silicon, carbide of calcium, and the silicides of the alkaline earth metals.

The carbide of silicon, or SiC, or carborundum, as it is commonly called, was discovered by Acheson about 1893. It is the direct combination of two very similar elements and probably has the structure Si≡≡≡C, the silicon and carbon being linked together by double bonds. It is only a shade less hard than the diamond and has found a ready market as an abrasive and is being manufactured on a large scale by the Carborundum Company at Niagara Falls. Very little of practical value has been discovered regarding its chemical properties, although it will undoubtedly be found to have some striking and very useful chemical properties which sooner or later will find commercial application of value.

The carbide of calcium, which was first produced commercially by Willson about 1895, we are all familiar with, chiefly from its use in the production of acetylene gas for illuminating purposes. Its importance as a reagent in manufacturing chemistry is, however, becoming recognized, and with the proper investigation of its properties it is only a question of time before its consumption for manufacturing purposes will equal, if not exceed, its present consumption as an illuminant. It is a powerful dehydrating

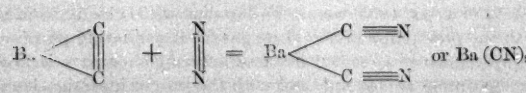
agent and possesses the property of extracting water and even the hydroxyl group from other compounds.

The possibility of using calcium carbide as a means of synthetically producing the valuable constituent of coal tar in a pure state, and the compounds of the benzine series in general, is very inviting; a very pure sample of anthracene, one of the valuable dye products, has been prepared by the Ampere Electro-Chemical Company, using calcium carbide as the original source of the hydrocarbon.

The carbides of the alkaline earth metals in particular offer a fruitful field for original investigation and synthetic work. The fact that they are unsaturated compounds and are capable of forming further additional products, under the proper conditions, makes them specially attractive.

All the evidence seems to point to (in which R represents the alkaline earth metal), as the structural formula for the alkaline earth carbides, but two of the eight bonds of the two carbons being satisfied by the bivalent alkaline earth metal; the other six are satisfied by the affinities between the two carbons for each other, which is not a very strong combination. If now this compound be heated in the presence of some element having a stronger affinity for carbon than carbon has for itself, the bonds between the carbons will open and the new and stronger element will attach itself to the carbons of the carbide to form a saturated compound. Nitrogen, for instance, if passed through heated carbide of barium under the proper conditions will, at a certain critical temperature, combine with the carbide, forming a cyanide of the barium.

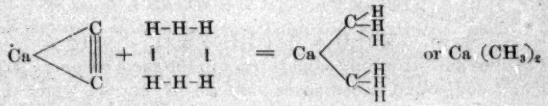
The structural equation probably best explains what takes place:



The Ampere Electro-Chemical Company has taken advantage of this property of the carbides in the production of the

cyanides of sodium and potassium, barium carbide being used as the absorber of nitrogen derived from the atmosphere, and as the carrier of the cyanogen, which is subsequently converted by ordinary chemical means into sodium or potassium salt, the barium being used over again in the production of more barium cyanide.

Hydrogen, if introduced with heated carbides, should combine in the same manner at some critical temperature, forming, for instance, with calcium carbide, what might be called calcium di-methyl.



Any of the unsaturated carbides should act in a similar manner.

The silicides of the alkaline earth metals, namely: calcium, barium and strontium, were discovered by the Ampere Electro-Chemical Company in July, 1899. These compounds which have the formula $Ca\,Si_2$, $Ba\,Si_2$, and $Sr\,Si_2$, respectively, are the silicon analogues of the alkaline earth carbides. The relationship is readily seen when the structural formulæ are compared:

Silicon has replaced the carbon of the carbide, R remaining the same in both compounds.

These compounds are formed in the electric furnace under conditions similar to those maintained in the manufacture of alkaline earth carbides, although at a somewhat higher temperature, when the carbonates, oxides, sulphates or phosphates, of the alkaline earth metals are mixed with silica, in which the relative proportions of the alkaline earth metal to the silicon in the mixture is as one to two, and sufficient carbon to effect the reduction is added; or when silicates of the alkaline earth metals in which the molecular relation of the alkaline earth metal to the silicon is as one to two, are mixed with sufficient carbon to

combine with the oxygen of the silicates and are heated in the electric furnace. These compounds are white or bluish white substances of metallic appearance, and resemble the silicide of aluminium and metallic silicon somewhat in appearance. They possess a distinctly crystalline structure showing plate-like crystals on fracture. They oxidize slowly in the air at ordinary temperatures, and more rapidly under the influence of heat, yielding silicon dioxide and the oxide of the alkaline earth metal. Like the carbides they decompose with water, but yield instead of acetylene, hydrogen in a pure state, which is evolved without explosion. The following is the reaction between barium silicide and water:

$$Ba\,Si_2 + 6\,H_2O = Ba\,(OH)_2\,2\,Si\,O_2 + 10\,H.$$

Calcium and strontium silicide react in the same manner. The calcium compound decomposes very slowly in cold water, but more rapidly in hot water; barium silicide decomposes rapidly in both hot and cold water, while the strontium compound decomposes more rapidly than calcium, but less so than barium silicide.

All these compounds produce large volumes of hydrogen in a pure state; when chemically pure the following amounts of hydrogen may be obtained from one pound of silicides on treatment with water:

1 lb. calcium silicide, 18.73 cu. ft. of hydrogen
1 " strontium silicide, 12.36 " " " "
1 " barium silicide, 9.15 " " " "

at zero degrees C and 760 M.M.

The silicides decompose rapidly in dilute acids, the barium compound producing silico-methane and free hydrogen according to the following equation:

$$2\,BaSi_2 + 4\,HCL + 6\,H_2O = 2\,Ba\,Cl_2 + 2\,SiH_4 + 4\,H + 2\,SiO_2 + 2\,H\,O.$$

Calcium silicide, strange to say, acts in an entirely different manner when treated with dilute acids and gives rise to the formation of a new chemical compound which has the formula Si_2H_2, the reaction which produces it being as follows:

$$Ca\,Si_2 + 2\,HCL = Ca\,Cl_2 + Si_2H_2.$$

This compound is the silicon analogue of acetylene, C_2H_2, and must therefore be called silico-acetylene, since it bears the same

relation to silico-methane, $Si H_4$, as acetylene bears to methane, CH_4. It is a yellow crystalline solid, and differs in properties from the compound Si_2H_3 which Ogier obtained by sparking silico-methane. The compound of Ogier was unstable and exploded when subjected to a shock. Silico-acetylene is not explosive and is stable at ordinary temperatures. Heated in the air it oxidizes rapidly, giving silicon dioxide and water. When heated in a closed tube, it breaks down into amorphous silicon and free hydrogen. It is insoluble in acids, but when treated with caustic soda and potash solutions it yields hydrogen and a silicate according to the following equation:

$$Si_2 H_2 + 4 Na OH + 2 H_2O = 2 Na_2 SiO_3 + 10 H.$$

Strontium silicide is remarkable on treatment with dilute acids, from the fact that it follows both calcium and barium in its behavior, yielding a mixture of free hydrogen, silico-methane and silico-acetylene.

The practical applications of the alkaline earth silicides and of silico-acetylene are many and varied, although the compounds are of almost too recent origin to have touched on more than a few of the most apparent of their uses. Barium silicide seems to be rather the most important of the silicides on account of its great activity and the positive nature of all its properties. Strontium would answer as well for most purposes, but the scarcity of strontium compounds makes it somewhat more expensive and difficult to obtain in commercial quantities.

As a hydrogen producer, barium silicide probably offers the cheapest and most convenient source of pure hydrogen in commercial quantities, it being only necessary to introduce the material into an ordinary acetylene gas generator and add cold water to obtain pure hydrogen.

All the silicides are strong reducing agents for both organic and inorganic compounds.

Barium silicide has been used for the reduction of indigo and seems to offer many advantages over zinc dust and Schutzenburger's reagent. It is only necessary to make the indigo blue into a thin paste with water and introduce the finely ground silicide; the solution of indigo white thus produced is clean and free from impurities and may be applied directly to the fibre of the cloth or substance to be dyed.

For organic compounds in general, when it is of advantage to make reductions in neutral solutions, barium silicide serves the purpose admirably.

Silico-acetylene may be substituted for barium silicide as a reducing agent when the reductions are carried out in caustic soda or potash solutions.

The silicides of barium and strontium will undoubtedly find many applications as reducing agents in the analine industry.

One of the large consumers of calcium, as well as barium silicide, will probably be the iron and steel industry. The alkaline earth silicides when melted with iron carrying sulphur and phosphorus have the property of combining with the sulphur and phosphorus, forming sulphides and phosphides of the alkaline earth metal, while the silicon alloys with the iron as silicide of iron. If now the melted mass be blown with air in a steel converter, the sulphides and phosphides of the alkaline earth are oxidized to sulphates and phosphates, while the silicon burns to SiO_2 and these impurities rise to the surface as slag, thus freeing the iron or steel of their sulphur and phosphorus. Tests have been made on irons carrying an excess of sulphur and phosphorus in which the last traces of these troublesome impurities were found to have been removed.

The silicides also offer a good field for original investigation. Tucker and Moody, in a paper published in the *Journal of the Society of Chemical Industry*, October, 1901, on the production of ethylene from inorganic sources, show that when a mixture of barium carbide and barium silicide is melted and the mass treated with water, a gas is produced which contains about 15% by volume of ethylene, and conclude their paper with the statement that it seems possible by the methods they employed to obtain a product which, on treatment with water, simultaneously produces a mixture of ethylene and acetylene. The ethylene is probably produced by the combination of acetylene and hydrogen rendered nascent at the same instant from the carbide and silicide in such close contact as to constitute practically a galvanic couple between the two compounds. This seems to be the most reasonable explanation of the results, since mechanical mixtures of carbide and silicide fail to produce the result.

A practical suggestion offered by the results obtained by Tucker and Moody is that a mixture of acetylene and ethylene might work to very good advantage in gas engines, particularly for automobiles and launches, and the fact that the gas-producing compounds may be stored and conveniently carried in considerable quantity makes the suggestion rather attractive.

The electric furnace has to its credit the commercial produc-

tion of two of the products of nature which are worthy of mention, namely: the production of graphite from amorphous carbon or coal by Acheson, and the production of corundum from amorphous alumina, such as bauxite or other aluminus compounds, invented by the Ampere Electrochemical Co. and now being operated by the Norton Emery Wheel Co. Both of these processes are in commercial operation at Niagara Falls.

The advantage to the consumer of being able to secure manufactured graphite and corundum in unlimited quantity, and of a higher degree of purity than the natural products, at a competitive price, is of inestimable value commercially, especially since the natural sources of both of these products are, as a rule, isolated from the points of consumption and the natural supply is becoming somewhat limited.

So much for the electric furnace and its new products and their probable value in industrial chemistry. The electric furnace, is, as a rule, only of commercial value in carrying out processes or producing results that cannot be obtained in any other way. The enormous consumption and cost of horse power required for chemical work and the consumption of carbon electrodes are items of serious expense. There are, however, a number of instances in which it is of advantage to introduce the electric furnace to replace the ordinary means of bringing about chemical reactions that take place at high temperatures, since it not only shortens the time required for the reactions and thus increases the yield for a given time, but the reactions in many cases take place much more completely. It also offers the additional advantage of placing the heat at the point required. The substitution of the electric furnace for the ordinary means of bringing about heat reactions is, as a rule, merely a problem of economic engineering.

Working under the Readman process, it has been found advantageous to introduce the electric furnaces in the manufacture of phosphorus; the retorts containing the ordinary mixture of phosphorus-bearing material, silica and carbon are heated from within by an arc, and the phosphorus is distilled off and is condensed under water in the usual manner. The chief feature of the process is economy in breakage of apparatus, due to the external heating by the old method; for the same reason electrical heating has been resorted to in the manufacture of carbon bisulphide. However, the more perfect control of the heat has also been a feature in both of these instances.

Another important use of the electric furnace in industrial chemistry is its application in the treatment of substances which are difficult to reduce, or which yield only a partial reaction even on long continued treatment at temperatures obtainable in ordinary combustion furnaces.

The old method of working barytes or barium sulphate consists in treating it with coal or coke in reverberatory or retort furnaces. By this method from 65% to 75% of the available barium sulphate is converted by reduction into the soluble sulphide of barium, which is then manufactured with other salts of barium.

By a process in operation by the United Barium Company at Niagara Falls, barytes is treated in the electric furnace, whereby from 97% to 99% of the available barium sulphate is converted into soluble form.

The electrical process was invented and developed by the Ampere Electro-Chemical Co. Its discovery brought to light a new chemical reaction which takes place beyond the range of temperatures attainable in combustion furnaces.

When a mixture of three molecular equivalents of barium sulphate and one molecular equivalent of barium sulphide are introduced into the electric furnace and the mass brought to a state of fusion, a reaction sets in, according to the following equation:

$$3BaSO_4 + BaS = 4BaO + 4SO_2.$$

The reaction takes place so completely that not more than two or three per cent. of the unconverted sulphate is left in the charge, and taps have frequently been made from the furnace in which the amount of unconverted barium sulphate present was found to be less than one per cent.

The furnace used in the process is a new form of continuous electric furnace of the direct-heating arc type and is tapped periodically, similarly to an iron blast furnace. The product tapped from the furnace is dissolved in hot water and the insoluble impurities filtered off. The filtrate, which consists of a saturated solution of barium hydrate containing a small amount of barium sulphydrate, is cooled and the hydrate crystallized out as $Ba(OH)_2 + 8\,H_2O$. The sulphydrate remaining in the other liquors is separated from the crystals of hydrate and carbonated or converted into some of the acid salts of barium.

Barium hydrate is the chief product of the process, and on ac-

count of its extreme purity and the price at which it is produced by the electrical process, has met with a ready sale in fields which have heretofore been closed to it on account of price. The old methods of working barytes do not produce barium hydrate at a price which admits of its large consumption in the arts. The fields into which it is finding its way are the white pigment and dry color trades, the purification of water for industrial purposes, and general manufacturing chemistry. Its largest consumption however lies in the beet sugar industry, for the purpose of recovering the sugar which remains uncrystallized in the molasses. In addition to the domestic consumption for this purpose, some large orders have been booked for foreign consumption. So steadily has the demand for this product increased, that after less than a year's operation it has been found necessary to increase the capacity of the plant to a basis of 60 tons of barium hydrate per day.

A further great advantage of the electric furnace is the facility with which operations may be conducted in the presence of various gases so as to bring their chemical actions into play on other substances at a high temperature, or to make them react together, under the influence of the arc. Some rather striking results have been obtained by the use of hydrogen or gases containing a high percentage of it, such as water gas, upon various substances, particularly silicates, at the temperature of the electric furnace.

If kaolin or china clay be raised to the temperature of the electric furnace, and hydrogen be passed through the highly heated silicates, the hydrogen combines with the silicon to form silicon hydride, according to the following equation:

$$Al\,Si_2O_7 + 8\,H = SiH_4 + 2\,H_2O + Al_2SiO_5.$$

The reaction is progressive and if continued the lower silicate first formed will eventually be reduced to aluminium oxide. Metallic aluminium has even been produced byth is reaction by continuing the operation with the aluminium oxide, but owing to the high temperature at which the reaction takes place, most of the product is lost by volatilization and the yield of the metal is very small. The lower silicate of aluminium and the oxide formed make very good abrasives with a hardness varying from 8 to 9 in the scale.

The silicide hydrogen issuing from the furnace ignites spontaneously as it comes in contact with the air-producing silicon

dioxide and water vapor. This silicon dioxide being the product of the combustion of a gas is in a state of extreme subdivision and makes a polishing agent for silver and fine metal work superior to most of the products in use for that purpose.

Probably no field offers so many important commercial problems for solution as the chemistry of the gaseous elements. The possible synthetic combinations of gases and combinations of the gaseous elements with solids open an almost limitless field for investigation, including as it does the possibility of the synthetic production of breadstuff, starches, gums, carbo-hydrates and the fertilizers. Undoubtedly the electric furnace and the electrolytic cell will play an important part in the solution of many of these problems. Sir William Crookes pointed out the importance of the atmosphere as a source of nitrogen compounds for the production of fertilizers. Crookes, Lord Rayleigh and others have shown experimentally that chemical combination of the nitrogen and oxygen of the air can be made by means of a disruptive discharge. That their results have remained experimental, is largely due to the fact that their conditions lacked one great essential, namely,—energy. The disruptive discharge has little or no energy. To bring about chemical reactions in anything but trivial amounts, requires a definite amount of energy. It remained for Mr. Charles S. Bradley to take the initiative in grasping the fallacy of the experimental results, and to introduce the energy necessary to bring about the combination of the oxygen and nitrogen of the atmosphere in commercial quantity. The work carried on jointly by Messrs. Bradley and D. R. Lovejoy, first in New York City and for the past year at Niagara Falls, has carried the problem of the fixation of atmospheric nitrogen from the realm of experiment and placed it on a basis of commercial development.

Mention of the process is not out of place in this paper, since in reality the apparatus in which the combination of the nitrogen and oxygen takes place may very properly be classed as an electric furnace of the direct-heating arc type, in which the arc imparts its heat directly to the charge, which in this case is dry air.

The Atmospheric Products Co., with which Messrs. Bradley and Lovejoy are connected in this work, depends for the combination of oxygen and nitrogen upon the proper use of a number of extremely high tension arcs brought into intimate contact with carefully dried air. In addition to the high tension of the arc, sufficient current is carried to furnish the necessary energy for

raising the molecules of nitrogen and oxygen to their ignition or combining point.

The results so far obtained have demonstrated on a practical scale great economy in the manufacture of nitric acid and the nitrates, also nitrous acid and the nitrates, which makes possible the cheap production of all the commercial products which can be chemically derived from these compounds.

Of the value of the electric furnace in purely scientific investigations little need be said. As someone has already pointed out, in the formation of the metallic carbides and their subsequent decomposition, the electric furnace has offered a very plausible explanation of the occurrence of our natural gas, petroleums and bitumens, while the formation of graphite and corundum has thrown still further light on some of the phenomena and products of the various geological ages.

A paper presented at the 162nd Meeting of the American Institute of Electrical Engineers at New York, February 28th, 1902.

ELECTROLYTIC CONDUCTION WITHOUT ELECTRODES.

BY CARL HERING.

Althcugh considerable is known concerning the theory of electrochemical action, there are still many points about which there is not yet indisputable, positive knowledge. About Faraday's law, which is one of the chief foundation stones of all electrochemical actions, there is fortunately no doubt, provided this law is correctly stated, which is not always the case; but about some other fundamental laws which are also important, though less so than Faraday's, our knowledge is not yet of a positive kind. Certain questions, for instance, arise from the fact that *two* electrodes have always been required in every electrochemical action, and all we know concerning certain numerical values, like the e. m. f., for instance, is the algebraic sum of those at the two electrodes, as distinguished from the actual values at each. Electrolysis with only *one* electrode has never yet been possible, and will probably always remain impossible, for if electrolytic conduction is characteristically different from conduction in solids, then in every circuit in which a current flows from one electrode to one electrolyte, there must of necessity alway be a second point at which the electrolytic conduction changes back into metallic conduction. Or, in other words, such a thing as a gradual change in imperceptibly small steps from one to the other must always remain impossible.

Other questions in the theories of electrochemistry arise from the fact that it has always been found necessary to use electrodes in order to pass a continuous current through an electrolyte, and

it is therefore claimed by some that electrodes must of necessity form a part of every electric circuit containing an electrolyte, or in other words, that electrolytic conduction without electrodes is impossible, a conclusion with which the writer does not agree. Some even go so far as to define electrolytic conduction by the action at the electrodes, and according to such a definition there would be no electrolytic conduction (as distinguished from metallic conduction) if no electrodes were used. It is, therefore, of no small interest and sometimes it may be of great importance, to know whether electrolytic conduction without electrodes, is possible or not, and to be able to make experiments under such conditions.

It has been suggested to form a circuit entirely of electrolytes separated by porous cups, and choose these electrolytes so that the chemical combination of several of them produces an electromotive force sufficient to send a current through the whole circuit. Whether such a combination is theoretically possible is still questioned; as far as the writer knows, no one has yet succeeded in making just such a circuit, although probably many have tried. It is claimed by some that the direct and counter e.m.f.'s would always add up exactly to zero, just as they do in a closed thermo-electric circuit, the whole of which is at the same temperature, and in which a current would mean perpetual motion. Such an analogy, however, does not seem to be correct, as there is energy set free by many combinations of chemicals. The nearest approach to such an electrodeless circuit seems to be the so-called "liquid element" in which "reversible" electrodes are used at the two ends of a series of electrolytes, that is, electrodes at both of which the chemical change is identical but in a different direction, so that whatever energy is consumed at one, is set free again in exactly the same amount at the other. Copper electrodes in copper sulphate, zinc in zinc sulphate, mercury with calomel in chlorides, etc., are reversible electrodes. Such current-producing cells have been described by Nernst[1] and by Worm Mueller.[2] Evidently the electrodes were used merely to be able to connect the cell to a galvanometer in order to measure the e.m.f. and current, and if the two ends, which always contained the same electrolyte, had been brought together by using a circular trough with porous partitions, and the electrodes removed, a current ought to circulate. For if the

1. *Zeitschrift fuer Physikalische Chemie*, Vol. 4, p. 129, 1889.
2. *Planck: Wiedemann's Annalen*, Vol. 40, p. 561, 1890.

actions at the two electrodes are exactly the same but opposite in direction, the cell ought to give current just as well as if they were removed. Nernst's device, however, was really a concentration cell, the energy of which was not obtained from true chemical combinations, but from the heat of solution or dilution of substances in water.

An unquestionable solution of the problem of conduction without electrodes which has been suggested frequently and has been tried, consists in placing a circular trough around the core of a transformer and inducing an alternating current in it, by making it the closed secondary circuit of the transformer. This will undoubtedly operate, and in fact was tried with success by Behn-Eschenburg for measuring electrolytic resistance.[1]

To show that a current actually flows under these conditions, and to measure it without using electrodes, it has been suggested[2] to make this trough pass around the cores of two separate transformers, so that it is the secondary of one and the primary of the other; by measuring the induced current in the metallic secondary of the second, the current which has passed through the electrolyte could be calculated.

Another method would be to fill a rubber hose with the electrolyte and wind it around the transformer cores, or pass it around an alternating current galvanometer just as though it were a wire. Another method of showing the presence of the current and measuring it without electrodes, is to measure the primary when the electrolytic secondary is closed, and then when it is opened by emptying the tank or inserting a solid insulating diaphragm. Or by replacing the tank with a wire of the same resistance and measuring the current in it, after adjusting it so as to make the primary current the same. These methods are suggested because it has been claimed by some that without electrodes it would be impossible to find out whether the current really flowed or not.

Unfortunately, however, the current produced in the electrolyte by a transformer, is of necessity an alternating one and there are, therefore, no chemical changes in the circuit; for this reason it seems that it cannot be used to settle the question of whether chemical changes, either by substitution or by true decomposi-

1. *Zeitschrift fuer Elektrochemie*, Mar. 2, 1889. *Elec. World and Eng.* April 8, 1899, p. 448. *Electrician*, London, June 2, 1899, p. 202.

2. By Kahlenberg, I believe.

tion, can be produced by electrodeless conduction. Those who believe it cannot, are therefore strengthened in their belief.

It has been thought by some to be an impossibility to devise any apparatus in which a direct current could be produced continuously in an electrolyte without electrodes. As far as the writer can find out, no such apparatus has ever been used, and as it would be of interest, it is safe to assume that if it had been used, descriptions would have been published. Korda describes an alleged unipolar induction apparatus,[1] but it is easily seen by applying the well-known laws of induction, that he has made a mistake, like many other designers of unipolar machines, as the current will not flow as he thinks.

Von Heuser[2] describes an apparatus in which there is unipolar induction. He makes the electrolyte flow in a very rapid stream through a magnetic field. He suggests that it might be used in commercial electrolysis in which the electrodes, which must be used in the ordinary method, are soon destroyed by such products as oxygen and chlorine. His mistake, however, seems to lie in the fact that he believes that the gases will be liberated where a sufficient fall of potential exists, even though there are no electrodes there. It is hardly likely, therefore, that this method of induction could be used for commercial electrolysis.

This brings up the question of the location and nature of the chemical reactions in such a circuit, supposing a current to exist. It would undoubtedly be at the junctions of the different electrolytes; but suppose a gas was evolved by the combination of the two ions which meet there, would gas bubbles rise out of the liquid there? This would be an interesting experiment if it could be devised. The writer has obtained a gas on the surface of a porous partition separating two electrolytes; it was thought at the time to be vapor due to heat, but this would have been condensed immediately in the cold liquid. Unfortunately, the record of this experiment was lost. Another interesting question is whether there would be a similar "skin effect" in electrolytes with high frequency currents, as there is in solids.

A simple, but slow method of generating an intermittent direct current would be to take the same transformer device above described, but to use an intermittent direct current in the primary, and open-circuit the electrolytic secondary by inserting

1. *Electrician*, and *Elec. Rev.* London, June 30, 1899; *Elec. World and Eng.*, July 22, 1899, p. 135.

2. *Elektrochemische Zeitschrift*, June, 1900.

an insulating diaphragm every time the primary current is broken.

In view of the fact that it has often been claimed to be impossible to induce a continuous current in an electrolyte without electrodes, and in view of the fact that some interesting theoretical questions concerning the electrolytic conduction might be settled by means of such an apparatus, a description of the following, which was devised by the writer some time ago, may be of interest.

The apparatus is in principle a mere unipolar machine in which the current is induced in a column of the electrolyte, instead of in copper bars or disks, as usual. The circuit of this current is

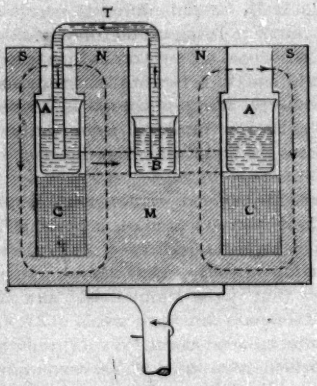


FIG. 1.

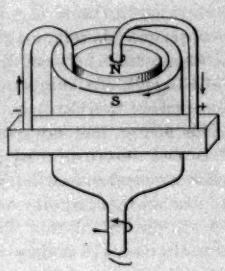


FIG. 2.

then closed through one or more electrolytes, without any electrodes.

Referring to the adjoining Fig. 1, which shows a vertical section, and Fig. 2, a side view of a modification, M is an ironclad electro-magnet with its coil at CC, and the annular air-gap at S N and N S; the paths of the lines of force are shown dotted. It also contains a circular tank, A A, which is connected by means of tubes with the middle tank, B; these are filled with the electrolyte. The whole magnet, with its tank, is revolved. A stationary U-tube, T, also filled with the electrolyte, is inserted as shown, so that one limb passes through and is cut by the magnetic field.

A continuous current will then flow through the liquid circuit; the direction will be as shown if the conditions are those indicated. As the tank revolves with the magnetic field, there will be no induction in the return circuit and the action will be that of a true unipolar machine, so-called. Unfortunately, in all such machines there must always be two sliding contacts, which, in the present case, are where the U-tube enters the tanks. With liquids these may give rise to some inconvenience, but if the ends of the tube are surrounded by porous cups there ought to be no serious trouble due to the pressure produced by the rapidly-moving liquid.

The illustration is only a diagrammatic one, unessential details being omitted for the sake of clearness. In its actual construction the proportions would naturally be quite different.

The top of the stationary tube, T, being visible, may be filled with various electrolytes either separated by porous partitions or contained in mineral gelatine, and the action of thecu rrent may be observed therein. If, however, an open trough is desired, the arrangement shown in Fig. 2 might be used; in this the ends of the trough are connected with the liquid circuit in the machine by means of the U-tubes.

The writer has not constructed the apparatus, but merely offers the description of it here to show that it is possible to devise an apparatus in which a continuous direct current can be induced in a circuit consisting entirely of electrolytes.

A simple calculation shows that if the annular field is made about 40 c.m. (16 inches) in diameter, then with a field of 17,000 lines per sq. c.m. there would be induced about 1.8 volt for every 10 c.m. height of the annular field, at a speed of 500 revolutions per minute. As the voltage required to circulate a current in a complete circuit of electrolytes is probably only very small, it would be quite possible to generate it with such an apparatus. It would even be possible to generate sufficient voltage for most decompositions if it were possible to bring about decomposition without electrodes.

The question arises how to detect a current in such an apparatus. One way would be to separate the circuit into parts by porous partitions and place in one of these compartments sulphate of copper and in the two adjacent ones sulphuric acid. Then the blue copper sulphate ought to move into one or the other of these compartments, depending on the direction of the current. Another method is to mix the different electrolytes

with mineral gelatine and then place them side by side in a glass trough; the current ought then to produce visible changes at their surfaces of contact.

Such an apparatus would show whether or not such conduction in electrodeless circuits were possible. With a series of electrolytes separated by diaphragms there would then necessarily be changes of composition due to the exchanges of the different ions; there would be what the chemists call mere substitution, such as when precipitates are formed by the mere exchange of atoms, in the mixture of different compounds. Whether or not there can be true decomposition, such as reduction and oxidation (or perduction) which takes place at all electrodes, is quite a different matter, and should therefore not be confused with the other. It is quite an interesting question whether such decomposition would be possible in an electrolytic circuit without electrodes, as it would have an important bearing on the modern theories of electrolysis. It is not apparent how such decomposition could be possible in the light of present theories, and therefore there is no prospect of such a method ever being used commercially for decomposition without electrodes, as was suggested by Heusser, and perhaps others also. For bringing about chemical substitution, as distinguished from true decomposition, such a method might be made useful, but in that case it would probably be simpler to use reversible electrodes, rather than a somewhat cumbersome machine such as the one described above.

In the writer's opinion the above-described apparatus need be used only to settle the first question; that is, whether electrolytic conduction is possible without electrodes, or, in other words, is the action which takes place in the electrolyte independent, or dependent on what takes place at the electrodes. All further questions could then be investigated more readily with the aid of like reversible electrodes, as Nernst and others used.

A paper presented at the 162d Meeting of the American Institute of Electrical Engineers, New York, February 28th, 1902.

LIQUID POTENTIOMETER; DETERMINING ELECTROLYTIC RESISTANCES WITH DIRECT CURRENT INSTRUMENTS.

BY CARL HERING.

In a certain case which recently occurred in the writer's practice, the following problem presented itself. A number of electrolytic tanks were connected electrically in series. But for filling, circulating and emptying, they had to be piped to a common tank. These pipes practically connected in multiple all the liquids in the tanks; hence the tanks, being connected simultaneously in both series and multiple, were all short-circuited. The conditions were such that the usual methods of breaking the continuity of these columns of liquids in the pipes, by means of air bubbles, stop-cocks, successive filling, etc., were by their nature excluded, and the only thing left was to make the resistance of the columns of liquid in the connecting pipes so great that the loss by the leakage current through them, would be negligible. This is always possible, even for a given rate of flow of liquid in the pipes, and given mechanical pressure, because the mechanical resistance to the flow of liquids in pipes decreases more rapidly with an increase of diameter, than the electrical resistance; or in other words, for a given electrical resistance, the flow of liquid with the same head, increases with the diameter of the pipe. In this particular case, however, nothing but lead pipes could be used; and when the fall of potential in the liquid column was made relatively great, the current would naturally leave the high resistance liquid, pass into the low resistance pipe and back again

at the other end of it; the object of giving the column of liquid a high resistance would thereby be lost. This was overcome by breaking the electrical continuity of the lead pipe with insulating joints, and the question then arose, at what intervals must these joints be placed so that the drop of voltage in each section would be just below that required to pass from the liquid to the pipe and back again, as the lead pipe, although an excellent conductor compared with the electrolyte, would then act like a pipe of insulating material and confine the current to the electrolyte. In making tests to determine this maximum drop of voltage an apparatus was devised which incidentally led to the interesting fact that the principle of the potentiometer may, with certain precautions, be applied to liquids also, and that such an apparatus may

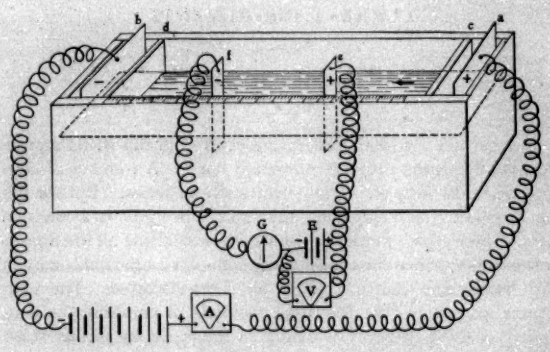


FIG. 1.

be used, among other things, for measuring the resistance of liquids with direct currents, and with only a simple direct current ammeter and voltmeter or galvanoscope, besides a cheap improvised apparatus.

The description of this liquid potentiometer and of the methods of using it, are as follows:

Referring to the adjoining figure, the apparatus consists chiefly of a tank of rectangular cross-section which it made of some insulating material. It should be accurately made, so that the cross-sections of the column of liquid in it are uniform throughout. It should for the same reason be carefully levelled. A scale is attached to one of the upper edges as shown. The tank

is filled to any convenient level with the liquid to be tested, and the cross-section of the column of liquid is measured. Any suitable electrodes *a* and *b* are inserted at the two ends, and to prevent the products of decomposition at these electrodes from altering the composition of the liquid under test in the middle of the tank, it is well to separate these electrodes from the rest of the tank by the porous partitions, *c* and *d*, or by placing them in porous cups. The fall of potential at these electrodes and the resistance of the porous cups do not affect the quantities to be measured, and they need not, therefore, be known and may be quite high. A suitable direct current is then passed through the column of liquid by means of these electrodes; it should be constant and must be measured in amperes.

Not considering the ends of the tank, there will then be a regular drop of potential along this column of liquid, like that in a potentiometer wire. If this drop could be measured in volts, between any two points, then the resistance of the column between those points could be determined by Ohm's law, and from this and the known cross-section and distance between the points, the specific resistance of the liquid could easily be calculated.

The difficulty lies in determining this drop of potential without introducing a large and unknown or variable error due to the direct or counter e.m.f., or what is often called polarization, at the electrodes which must be inserted in the liquid in order that a voltmeter may be used to measure this drop in volts. It would evidently be quite improper to use two platinum electrodes connected to a voltmeter, as the shunt current in the voltmeter, even though it is extremely small, would cause very appreciable errors in the form of direct or counter e.m.f.s, at the contact of the platinum and the liquid, as gases would be formed there. Even an electrometer could not be used in this way instead of a voltmeter, as the small charge entering the instrument would produce some chemical decompositions at the electrodes, and thus give rise to an error.

The only correct way is to have absolutely no current flowing in that voltmeter circuit. This is accomplished in a very simple way by inserting a cell, B, of known e.m.f. in this local circuit, taking care, of course, that it is properly connected with respect to its polarity. This need not be a standard cell, but may be any convenient accumulator or primary cell or group of cells whose voltage is measured at the time of making the test; whether it is essential or not that its e.m.f. be great enough to

decompose the liquid under test, for reasons which will be better understood later, was not determined; in the writer's tests it was great enough for such decomposition. Any convenient galvanoscope or zero-reading instrument, which may be a sensitive Weston instrument, is then inserted, as shown by G in this circuit, and the two auxiliary electrodes *e* and *f*, are then moved farther apart or nearer together until the current in that local circuit is zero. The resistance of the length of the column of liquid between these two electrodes is then equal in ohms to the quotient of the voltage of that cell by the current in amperes flowing through the tank. Knowing the cross-section of the liquid column, the specific resistance of the liquid is then easily calculated.

The nature of these two auxiliary electrodes for measuring this drop of potential is, however, of very great importance. They must, of course, be identical in material in order that the counter and the direct e.m.f. at their contact with the liquid, balance each other exactly and are thereby eliminated. They must also be chemically inert toward the liquid except perhaps in a case in which the liquid is a salt of a metal, when it might be possible to use electrodes of that metal, as copper in copper sulphate, because there is then no polarization, and the direct and counter e.m.f.'s are equal and opposite; or the so-called "hydrogen electrode" might be used with certain acids; but the writer has not tried these and cannot say whether or not they will answer the purpose. The electrode must furthermore not have the property of combining with the products of the slight decomposition which must take place before the point of zero current has been found, and for this reason platinum will not answer well in the many cases in which oxygen and hydrogen are evolved at these electrodes, because it at once unites in some way with these gases and then becomes useless as an inert electrode. Platinum black electrodes are, of course, still worse for the same reason. Carbon was found to possess this property to some extent also; moreover, it cannot well be had in such sheets that a sharp reading can be taken on the linear scale. It was found that with platinum, for instance, no definite points could be found (when the liquid was sulphuric acid) at which the current would be zero; the position for zero reading would depend upon which direction the current in that local circuit had just before; that is, whether the plates were being moved apart or toward each other; also, on how large that previous current had been.

After trying various metals, it was found that gold was by far the best and was very satisfactory, at least for that large class of electrolytes in which oxygen and hydrogen would be evolved at these electrodes. Two gold coins answered very well when their thickness did not introduce too great an error in the reading of the scale. Gold may possibly not act as well in solutions of chlorides or cyanides; the writer has not tried it in such solutions. It is important, of course, that these electrodes be only slightly immersed in the liquid, so as not appreciably to change the directions of the paths of the current around them.

It was found to be very necessary to approach the zero position from each of the two directions. The simplest way to do this, is to oscillate one of the electrodes very slightly when the zero position has been found approximately; if the needle of the galvanoscope then oscillates on *both* sides of its zero point, the correct position has been found. This method was the one used and gave very satisfactory results. Another method of effecting this oscillation would be to superpose a small alternating current onto the main current in the tank. This might be done by inserting an electro-magnet in the main circuit and vibrating an armature in front of it. Perhaps a current from a dynamo would be sufficiently pulsating; the writer used accumulators as a source. Another method would be to have a small external resistance in circuit with the tank, and to short-circuit it sufficiently rapidly so that the ampere meter will still indicate the mean current.

The deflections just before the zero position was found, seemed to be somewhat of the nature of slow condenser charges and discharges which gradually fell toward zero.

The writer used the apparatus only for obtaining results sufficiently accurate for practical purposes, and therefore is not able to say whether it could be used also as an instrument of precision for research work. For practical work it answers very well, and has the advantage that the ordinary direct current instruments, usually at hand, can be used, the only special apparatus being the tank, which for most liquids can be readily made of wood, coated on the inside with asphalt varnish. The resistances of liquids determined in this way agreed very well with those recorded in tables.

The apparatus was used in the following way to determine the greatest potential at which a current would not pass continuously from an electrolyte into and out of a lead pipe through which the

liquid flowed, as described in the introductory remarks. After having determined, as just described, the fall of potential per centimetre of column of liquid in the tank, the test electrodes were replaced by others of lead which were connected through a galvanometer or ammeter (without a battery). When these electrodes were gradually moved away from each other, no current flowed through this ammeter until they were a certain distance apart, and then current suddenly began to flow. Knowing the fall of potential per centimetre, and the distance between the electrodes when current suddenly begins to flow, the desired voltage is readily calculated.

The same apparatus might be used for other purposes also. For instance, to measure the resistance of diaphragms, like porous cups. The resistance of any convenient length of the column is first measured by itself and then with the diaphragm inserted, the increase being that of the diaphragm.

A modified method, which was tried prior to the one described above, proved to be a failure. It consisted in using two electrodes of *different* materals, such as zinc and carbon, for instance, when the liquid was sulphuric acid; these would by themselves produce an e.m.f. when no current was flowing in the tank; this e.m.f. was measured and the current in the tank was then started and increased (or the distance between the electrodes increased for constant current) until the drop of voltage in the column of liquid between these electrodes was just equal to the direct e.m.f. of the electrodes, in which case no current would flow from one to the other. The results, however, were very unsatisfactory and indefinite. The secret of the success of the apparatus lay entirely in the use of the supplementary battery and identical, inert or reversible electrodes.

The theory of the method does not seem to be clear, but as actual and correct results were obtained, there must be some correct explanation. The important points are, that the current in the auxiliary circuit must be zero, so that there can be no chemical decomposition or polarization in this circuit; and that the two electrodes must be alike, so that any possible and unknown e.m.f. that may exist between the electrode and the liquid, is eliminated by the fact that it is always exactly balanced by a second one of equal amount but opposite sign at the other electrode. The electrodes must furthermore be chemically inert in the liquid under test, with perhaps the exceptions above cited. The apparent conflict with theory arises from the fact that it

would at first appear that there could be no inverse current in the auxiliary electrodes until the fall of potential between the auxiliary electrodes is great enough not only to overcome the e.m.f. of the battery, but also that for the decomposition of the solution under test. But as such currents were really obtained, it shows that this explanation does not apply. Probably the currents obtained near the zero position are due to the condenser action of the auxiliary battery; in fact, they were more like throws of the needle than steady currents; it was found necessary to watch the deflection of the needle while the electrode was being moved, as the deflection seemed to diminish immediately afterwards. If this is the correct explanation, better results might possibly be obtained by adding a condenser in that auxiliary circuit, parallel to the battery. Or these currents may be what the Germans call " reststroeme," that is, currents which flow before the voltage of decomposition has been reached. Time did not permit making researches to settle this question. This is left to others who may be interested.

A paper presented at the 162d meeting of the American Institute of Electrical Engineers at New York, February 28th, 1902.

POINT OF CUT-OFF IN A BATTERY DISCHARGE.

BY CARL HERING.

In making a test of a battery to determine its output in ampere-hours or watt-hours, the question necessarily arises, when shall the discharge be considered ended. The voltage of all batteries falls as the discharge approaches the end, and if this fall were a sudden and definite one, it could be taken as the proper "point of cut-off" of the discharge; but this is the exception, the rule being that the voltage falls gradually, without any sudden bend in the plotted curve, and it is then difficult to say when the discharge shall be considered to be ended; the point of cut-off is then necessarily an entirely arbitrary one. As the output in both ampere-hours and in watt-hours keeps on increasing, no matter how low the voltage has fallen, much depends on this arbitrary selection of this final voltage, when one is measuring the output of a cell. This is especially important when the output of batteries of different voltages are to be compared with each other, or when comparisons are to be made between the discharges of the same battery at different rates. It is evidently unfair to a lead accumulator, for instance, to always stop its discharge at say 1.8 volts, on closed circuit, when the discharges are made at different rates, or even when one discharge is made at constant current and another at constant external resistance. It is evidently proper to allow a lower final voltage for the higher rates, but the question then arises, what shall the different lowest voltages be at these different rates of discharge.

In some cases the question settles itself. Batteries of the Leclanche or Daniell type, for instance, used on bells or similar work, are of use until their voltage is zero on closed circuit. In

practice their drop in voltage will have to be replaced by fresh cells, but as long as they add anything at all to the voltage of the circuit, they are still of use. Zero voltage on closed circuit would, therefore, seem to be the proper point of cut-off in testing such cells.

When used for lighting purposes, the point of cut-off is fixed by the lowest voltage which is still sufficient to run the lamps at their proper candle-power; all above that is of no use, and must be wasted in regulating resistance, unless end-cell regulation is used.

With lead accumulators the point of cut-off of the discharge is generally determined by that fall of voltage below which the plates would be injured. But it is not generally known what this voltage is for different rates of discharge. Nor would it necessarily be the same for different types of lead accumulators.

In still other cases there may be other reasons which determine the lowest voltage to which a discharge should be continued, but there seems to be no general rule which is applicable to all batteries. An approximation to a general rule is to stop the discharge when the voltage has fallen a definite fraction of the original, say 10%. The question then arises, what is to be taken as the original voltage; is it the e.m.f. on open circuit, or the difference of potential on closed circuit; if the latter, then at what time should that reading be taken, as there is always a rapid drop in the first few minutes. A good rule is to take as the original voltage, the difference of potential on closed circuit at which the curve begins to become nearly horizontal. But for a high resistance battery it would evidently not be fair to assume the same percentage drop for rapid as for slow discharges.

While there may be many cases in practice in which the differences pointed out above are too small to be of great importance, yet there are often cases in which it is very desirable to decide on some definite point of cut-off which is as fair to one as to another battery. This is the case when it is desired to compare with each other, the discharges of the same battery at different rates, or different batteries with each other, or different batteries at different rates.

The first thought was to try to find some curve, such, for instance, as that of an integral or a differential of some of the variables, which has a characteristic point like a zero, a minimum, a maximum, or an inflection point at the proper point of cut-off. A number of trials, however, gave negative results.

Whether or not such a curve exists, the writer is not able to say at present, but if it does, it would probably involve too much calculation to be useful in practice, or the drawing of a new derived curve, which is troublesome.

For the particular case of constant potential, however, a simple solution was found, and as it does not involve drawing a new curve, and admits of a simple graphical solution by a trial method, it seems to be well suited for practical work. This solution is limited, however, to the case in which the battery is to be used on a constant potential circuit, as, for instance, for incandescent lighting, in which case all the voltage above the final is useless, and must be taken care of in the dead resistance of a regulator. The point of cut-off thus found then also gives the

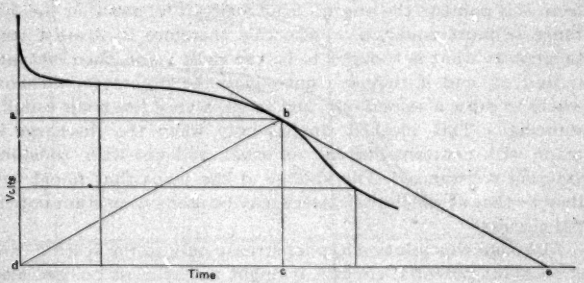


FIG. 1.

proper voltage at which this cell could be used for constant potential circuits; if, for instance, 110-volt lamps are used, then dividing 110 by the voltage thus obtained gives the number of cells required. If end-cell regulation is used, however, this method of finding the proper voltage, does not apply.

Referring to Fig. 1, which is an ordinary constant current discharge curve giving the voltages at successive times, it will be seen that the inscribed rectangles drawn at successive points, continue to increase from the beginning of the discharge up to a maximum and then diminish again. In this case the largest possible inscribed rectangle is *a b c d*. There is, therefore, a maximum of some quantity at this point. It will readily be seen that the area of this rectangle is the product of the volts and the

time, or what might be called volt-hours; but as the current is constant, this area is also proportional to the watt-hours. In other words, at that point the maximum output in watt-hours will be obtained; beyond that point the voltage falls so rapidly that the total useful watt-hours begin to diminish, owing to the fact that all the voltage above that is useless. The discharge may therefore be considered ended at that point, and the useful watt-hours, for constant potential circuits, will then be a maximum.

Locating this point by the differential calculus, that is, by making the first differential coefficient equal to zero, led my assistant, Dr. E. F. Roeber, to find the following very simple, graphical solution. It is a method of trials, but a very few trials generally suffice. He found that the angle of the tangent drawn at the point to be found, must be equal to the angle of the line from this point to the origin: from which it follows that the distance *ce* must equal *cd*. The rule therefore is, draw a trial tangent at what is thought to be the right point, then measure *ce* and *cd*, and if they are not equal, the difference will show where to draw a second one, and so on, a very few trials usually sufficing. This method applies only when the discharge is made with constant current, as usual, and not with constant external resistance. The voltage at the point thus found will then be that at which the battery may be used on constant potential circuits.

Although this solution applies strictly only to the specific case of constant potential circuits, it might nevertheless be used also in comparing different batteries with each other, or the same battery at different rates, even when they are not to be used on constant potential circuits, as it gives one a fixed definite point of cut-off, which is determined in the same way for all of them, and may be as fair for one as for the other. It is in any case better than an arbitrarily assumed point.

There is, however, another solution which may be still better; it is to discharge the battery at constant useful power; that is, at constant useful watts instead of at constant current or constant external resistance. This will be found to give a fixed and definite point, which is determined at once during the test, and requires no further calculations; the labor in making the test, however, is increased slightly. The current and the difference of potential at or near the start gives the power in watts; this power is then kept constant by so increasing the current during

the discharge that when multiplied by the diminished voltage it gives this constant number of watts. Toward the end of the discharge the voltage will fall rapidly and therefore the current will have to be increased rapidly; this in turn makes the voltage decrease still more rapidly, and a fixed and definite point will, therefore, soon be reached, when the current becomes so great that the battery is no longer able to give that power; this is the point of cut-off, and it is a very definite one, leaving nothing for arbitrary determination.

Such a discharge at constant power seems to be quite a rational one when the battery is to be used for power purposes, as on

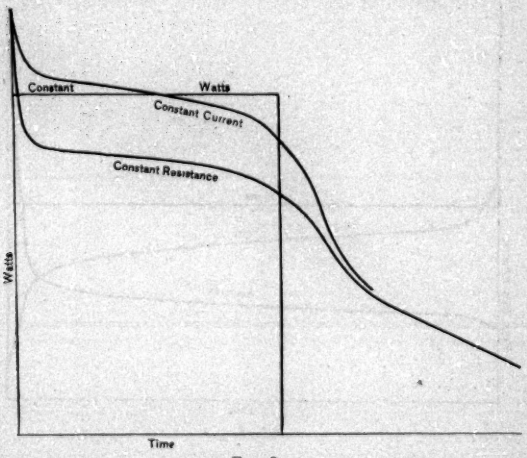


FIG. 2.

automobiles, for instance; but for any good battery the difference between this and a discharge at constant current will not be very great during the greater part of the discharge; the difference becomes great only for a short time near the end.

The test becomes somewhat more troublesome to make, for the reason that the current must be adjusted to correspond to the particular reading of the voltmeter, and the adjustment of the current again changes the voltage, thus requiring a re-reading of the voltmeter and a readjustment of the current; but with a little practice this is easily done. One ought, however, to calcu-

late at the start the current that corresponds to each reading of the voltmeter, so that he does not have to take the time to perform the calculations for each reading, but can simply take it from the table. Such a table is very easily calculated with a slide rule; or, the slide rule may be used instead of the table, as the values can be read off directly.

Fig. 2 shows the curves for each one of these three methods of discharge: at constant external resistance, at constant current, and at constant useful watts. The same primary cell was used in all three, and all other conditions were kept as nearly alike as possible. It will be noticed how difficult it would be to select

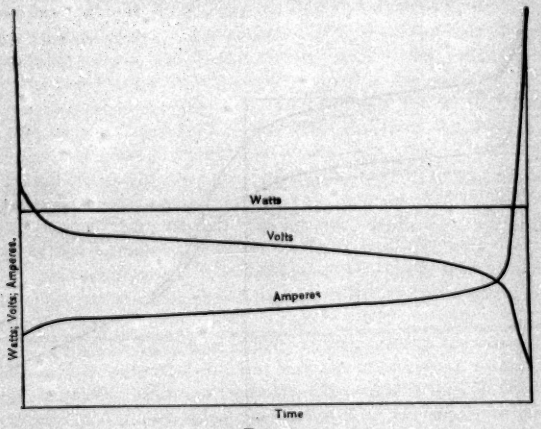


FIG. 3.

any definite point on either of the first two at which the discharge should be stopped, while for the constant power discharge the curve becomes a very definite rectangle, leaving no choice to the experimenter as to when to stop the discharge; it stops itself.

Fig. 3 shows the current curve, the voltage curve and the watt curve for this discharge at constant power. It will be seen how rapidly the current must be increased at the end of the discharge in order to maintain the watts constant as the voltage falls.

In making a constant current discharge of a few small cells, a simple and probably well-known method is to connect them to an ordinary constant potential incandescent lamp circuit in

series with as many lamps as are required to give the proper current. The current will then remain practically constant and needs no adjustment as the voltage falls. There is no advantage, however, in using such a lamp circuit for a discharge at constant watts, as one ought to be able to adjust the current by very gradual steps. A carbon rheostat of very low resistances forms the best external resistance to use for that test.

A paper presented at the 162nd Meeting of the American Institute of Electrical Engineers at New York, February 28th, 1902.

ELECTROLYTIC RECOVERY OF COPPER FROM LOW-GRADE ORES.

BY N. S. KEITH.

Low-grade cupriferous rocks are those which contain such small amounts of copper per unit (say a ton) of weight that a large number of such units must be treated daily to make a commercial profit.

Where labor, fuel, power, supplies and such requisites are cheap, and the market for the product is near at hand, the percentage of the yield of copper may evidently be less than in cases where expenses are higher, as in regions remote from civilization and the market.

Low-grade ores, may then, also be defined as those which contain from 1½% to 5% of copper, with favorable conditions in inverse proportion.

Some low-grade cupriferous rocks may carry enough gold or silver, or both, to make them valuable for treatment for recovery of all the metals when the amount of copper recoverable alone would be unremunerative.

There are two methods, the dry and the wet, by which low-grade cupriferous rocks are treated. In this paper I am to consider but one—the wet. There are varieties of the wet method, but I will confine myself to the one variety in which electrolysis is used to effect the final deposition of the copper obtained in solution during the procedure.

There are many varieties of copper ores which may be treated metallurgically by the wet method, but with many variations

which the limits of this paper will not permit of consideration.

A rehearsal of the conditions and considerations applied by the author in the planning and erecting of works for the electrolytic method of recovering copper from the ores of a mine a few miles from New York City, may be interesting and instructive.

These were the conditions:

The mine was worked intermittently from 1715 up to 1863; sometimes at a profit and sometimes at a loss, but always by methods crude and antiquated compared with those at present pursued in the art. All of the rock contains some copper, but there were, and are, in it streaks, seams, veins etc., which carry values up to 70% of copper, though these serve but to leaven the whole to the value of about 2%, or 40 pounds of copper to the ton; thus making millions of tons of low-grade copper-bearing rock which may by scientific mining and milling be profitably worked.

Prior to 1863 the miners sunk shafts and ran drifts in this rock without any apparent system, in the evident search for the richer ore seams, streaks and veins which I have mentioned. Whether they made or lost money depended upon their luck in finding more or less of these richer portions.

Modern methods frequently warrant the working of nearly or quite all of such rock containing disseminated rich ores rather than by picking out only the richer portions, solely for treatment, and throwing the major portions into the waste dump pile.

The ores impregnating the rock are *Chalcocite* (copper glance), Cu_2S, (copper 79.8%, sulphur 20.2%): *Malachite*, (green copper carbonate) (copper 62%); *Azurite* (blue copper carbonate) (copper 61%); *Cuprite* (cuprous oxide) Cu_2O (copper 88%), and some oxides of iron.

The rocks, called by miners "gangue," in which these ores are found, are of several kinds, mainly *sandstone*, of the Triassic period; *trap*, cutting the sandstone in all directions and frequently overlying it; *breccia*, *indurated clays* and *shales*. I will not attempt to theorize as to the origin of the copper contents of these rocks. They lie in a bed having a thickness varying from 20 to 60 feet, dipping westerly at an angle of about 9° from the horizontal. The mining is cheaply done, and the rock is dumped directly into the mill from the ore skip, which is brought from the mine by means of an electric hoist, and runs back into the mine by gravity. The scope of this paper does not admit of the consideration of the mining methods.

The problem for the metallurgist was to obtain the copper contents of the rocks after they were landed in the mill, in the cheapest and most expeditious manner. This was decided to be by dissolving the copper and depositing it from the solution by electrolysis.

In order to prepare the rock for treatment by solvents it is passed through the following operations:

As the rock is raised up the incline from the mine by means of a "skip" it is automatically dumped into the hopper of a Gates type of crusher, which breaks the rock to the size of about ¾-inch to 1-inch cubes, and smaller. Three minutes suffice for this operation on one skip load of one and one-half to two tons.

All of the machines of the plant are run by means of motors obtaining their current from a direct-current generator of 300 k.w. at 240 volts, direct connected to a tandem compound condensing engine. This generator also supplies the current for the electrolytic vats.

The broken rock falls from the crusher into a steel bin holding about 400 tons. This steel bin has its bottom inclined so that it may be emptied by gravity through gates on the side at the lower edge near the bottom.

The rock coming from the mine is damp, containing about 8 to 10% of water. Of necessity it has to be dried before further reduction in size. This is accomplished by forcing heated air or furnace gases through the body of ore lying in the bin. This has been found to be an economical and effective way of drying.

From the bin gates the rock is moved in a constantly flowing stream, by means of a feeder belt, to a set of crushing rolls, which further reduce the sizes of the pieces of rock to that of peas or beans, and smaller, even to sand and dust. This is then elevated and caused to run by gravity over inclined wire-cloth sieves, called "separators," which separate the sand and dust for the next step of treatment. The coarser pieces which do not pass through the meshes of the sieve run into pulverizing machines, which reduce them to the size at which they pass through the sieves when again run over the separators. The rock is thus crushed, ground and pulverized so that it will all pass through the meshes of a No. 30 wire-cloth having 900 holes to the square inch

This degree of comminution is necessary in order that the particles of copper ores impregnating the rock may be set free, or exposed, so that they may be acted upon by the agents used in

the next steps of the operation. These steps are roasting and lixiviation.

While the oxides and carbonates of copper are readily soluble in dilute acids, the cuprous sulphide, or chalcocite is not. Even strong and boiling sulphuric, or hydrochloric acid will not dissolve it. Boiling or hot nitric acid will. But the use of nitric acid is not admissible for other reasons than its expense.

Therefore it is necessary to decompose the chalcocite by expelling its sulphur and oxidizing its copper. This can best be done by roasting. The roasting takes place continuously, as the rock comes from the pulverizers, in a mechanical furnace having a hearth 200 feet long by 10 feet wide, and fired by coal in seven fireplaces arranged along its sides. Rabbles, or rakes, are caused to travel continuously from end to end of the furnace, in their course stirring the powdered rock, exposing continually renewed surfaces to the action of heat and atmospheric oxygen, and moving it to the discharge end. From the discharge end the roasted rock is conveyed to the lixiviation or leaching tanks. The capacity of this furnace is about 125 tons of powdered rock per day of 24 hours.

The reactions which take place in the furnace are these: The carbonates of copper are decomposed, setting free CO_2, and their copper contents oxidized to CuO; the chalcocite is decomposed, the sulphur set free and oxidized, and its copper contents oxidized to CuO; the ferrous carbonates are decomposed and the oxides are further oxidized, which makes them less soluble in dilute acids. The roasting of chalcocite, Cu_2S, does not produce any $CuSO_4$ as in roasting other copper sulphides, for instance, chalcopyrite, containing CuS. There is not sulphur enough in Cu_2S to supply the equivalent of S to form $CuSO_4$. The sulphur of the chalcocite either escapes with the gases of the furnace, or unites as SO_3 with the earthy alkaline constituent of the rock, lime, as calcium sulphate.

There is some calcium carbonate, as calcite, in the rock; say about ½ of 1%. In order to economize in the use of acid it is necessary to mix with the rock as it passes into the furnace some powdered sulphide ores, preferably carrying at least a small percentage of copper; which copper is recovered in the subsequent steps of the process. This is an economic feature; because such ores are cheaply purchased, and serve the double purpose of supplying sulphur to be oxidized and unite with the lime as $CaSO_4$, and a means of obtaining their copper contents. Sulphur

may be used in the same manner for the purpose of neutralizing or counteracting the lime. When sulphide, or sulphur, is used in the roasting, much of the copper is sulphated, and as $CuSO_4$ is readily dissolved in water.

The practical result of the roasting is a powdered rock having disseminated therein some $CuSO_4$, some CuO and some FeO and Fe_2O_3; the last of which list is practically insoluble in the acid used.

To extract the copper contents the solvent used is primarily $H_2SO_4 + x\ H_2O$. HCl may be used as a solvent, but there is a difficulty in obtaining anodes which will not seriously deteriorate in the subsequent electrolysis. The percentage of H_2SO_4 is determined by the amount of copper to be dissolved by a definite amount of solvent. The amount of H_2SO_4 may vary from 5% to 15% of the water of the solution.

As will be described further on, after the primary use of the solvent ($H_2SO_4 + x\ H_2O$), another solvent of the copper oxide takes part in the action: namely, $Fe_2(SO_4)_3$, ferric sulphate.

From the roasting furnace the rock is conveyed to one of four leaching tanks, filling them in succession. Each tank is cylindrical, 30 feet in diameter and 6 feet high, inside measurements. They each hold 125 to 150 tons of the powdered and roasted rock. The rock is put in hot as it comes from the furnace.

In the bottom of each tank there is a filter bed of coarsely crushed rock, covered with canvas, which is calked tightly around the edges and around the outlets, through which the powder is washed after the extraction of its copper contents.

From the center of the bottom of each tank, under the filter bed, a pipe, with a stop-cock, serves to draw off the solvent to the electrolytic cells.

During the filling of each tank a suitable amount of the solvent is admitted to moisten the rock; and finally, when filled to within about a foot of the top, the whole is flooded by the solvent to the depth of a few inches above the bed of powdered rock.

The solvent begins to run from the bottom of the first filled leaching tank as soon as filled, running through the filter bottom in a stream of perfectly clear, greenish-blue liquid. From time to time, as the liquid on the top is lowered to the surface of the rock, a few inches in depth more of the solvent, drawn from the "stock-solution tank," is run in.

The first solvent run in during the filling of the tank takes up nearly all of the copper, and runs out at the bottom fully satura-

ted with $CuSO_4$. As the fresh additions of solvent pass through the bed of rock they carry decreasing quantities of copper, until all of it has been taken up, and the solvent running out shows little or no more by assay than does the solvent run in.

But meantime tanks Nos. 2, 3 and 4 have been filled, and a like solvent action takes place in them. Then, in order to have a solution of nearly uniform value in copper to electrolyse, the richer liquors from No. 2 are run out simultaneously with the poorer from No. 1. This method is kept up in regular succession with the other tanks.

As soon as the rock in No. 1 has had its copper contents dissolved by the solvent the outlet is opened and the solution runs into the deposition vats; and as the quantity of copper in the solvent from this tank decreases in amount, the outlet of No. 2 is opened to allow a flow of strong solution to mix with that from No. 1; and so on with Nos. 3 and 4, and then from a new charge in No. 1, and so on, in a continuous cycle of operation. This procedure is for the purpose of supplying the deposition vats with a steady stream of lixiviate having constant percentage of copper.

The tanks are emptied by opening passage-ways through the filter bed and bottom of each tank, and turning a forcible stream of water, pumped for the purpose, against the depleted powder, then called " tailings," thus washing them out: after the manner called " hydraulicking " in some kinds of gold-mining.

The deposition vats are rectangular boxes of wood, protected from the action of the lixiviate, now the electrolyte, by a suitable lining. They are arranged so that the electrolyte flows from No. 1 through Nos. 2, 3 ,4, *et seq*, and finally out of No. 128, depleted of its copper, into a large tank, called the " Sump," from whence it is pumped up into the " stock tank " for re-use.

These deposition vats are so set, at differing elevations above the floor of the building, that the electrolyte runs by gravity from one to the next, and so on through the series to the end, through troughs, or spouts, between their ends, with this exception: As the vats are arranged in six rows, and one end of each row is at a greater elevation than the end next to it of the preceding row, it is necessary to raise the electrolyte from the one row to the next at those points. This is done by means of air-lift pumps, which are easily managed.

The electrical connections are as follows: The electrodes in each vat are connected in multiple, and the several vats of elec-

trodes are connected in a series of 128, exactly, so far, as is the case in large storage electric-vats.

But for the purpose of ensuring a retrogressive decrease of density of current at the electrodes of each vat, after the first of the series, the number of electrodes is increased progressively from the first to the last of the series. The amperes of current are the same in each vat of the series; but the current density is less and less in decremental order from the first to the last of the series, to compensate for the decrease of copper in the electrolyte in its course through the vats. Thus the density of current may be 15 to 20 amperes per square foot of cathode surface with a 6% copper solution, and 3 amperes per square foot with a 0.5% solution, provided proper circulation and sufficiently rapid movement of the electrolyte be kept up between the anode and cathode surfaces.

The evolution of free oxygen at the anodes keeps up a continual mixing movement of the electrolyte, in addition to the motion due to the flow through the vats.

During electrolysis there is a counter-electromotive force of 1.6 volts in each vat of the series- due to the difference of energy of combination between the materials on the surfaces of the anodes and cathodes, respectively, and the constituents of the electrolyte. The generator is operated at approximately 240 volts; which gives $240 \div 128 = 1.87$ per cell. The voltage per cell required is $1.87 - 1.60 = 0.27$ volt for resistance of conductors, etc.

The density of current must not exceed in any vat the limit beyond that required for the deposition of reguline copper.

Primarily, both the anodes and cathodes are of sheet-lead. But under the electrolytic action the anodes become coated with PbO_2, and the cathodes with copper. As soon as the copper deposit reaches the thickness of thin cardboard it is stripped off each lead cathode, which is then replaced in its cell, and the two copper sheets thus produced have suitable connections riveted to them, and they are then re-hung as cathodes in some of the vats, where they remain a sufficient length of time to receive a deposit of copper to the thickness needful for the market.

The author has tested anodes of carbon, both such as are used for battery plates, and the Atcheson graphite electrodes. There is much less oxidation and disintegration with the latter; but still too much for economy. There are, also, some other difficulties incident to the use of carbon. Under proper treatment

the lead anodes do not sulphate, and the formation of PbO_2 is merely superficial.

A certain proportion of the lead cathode plates are specially used for the purpose, as above, of producing the thin copper cathodes, it being a handier and a cheaper method than providing rolled thin copper sheets for the purpose.

The lixiviate running from the leaching tanks into the deposition vats consists of a solution in water of sulphuric acid, cupric sulphate and ferrous sulphate, expressed symbolically as follows:

$$xH_2SO_4 + 2CuSO_4 + 4FeSO_4.$$

The electrolytic action deposits the copper on the cathodes, perhaps by secondary action, without the evolution of hydrogen, but with the setting free at the anode of oxygen. But some of the oxygen and acid is appropriated by the ferrous sulphate, thus becoming ferric sulphate. When there is enough ferrous sulphate present at the anode, no free oxygen is evolved, but is appropriated by the ferrous sulphate to form basic ferric oxide, which unites with the sulphuric acid to form normal ferric sulphate, as per this equation:

$$x\,H_2SO_4 + 2CuSO_4 + 4FeSO_4 = 2Cu + 2Fe_2(SO_4)_3 + x\,H\,SO_4.$$

Taking the latter half of the above equation for consideration, the two parts of copper are deposited on the cathodes and the solution of ferric sulphate and sulphuric acid runs to the sump tank, from whence it is raised to the stock tank, and from there is used on subsequent lots of powdered and roasted rock in the leaching tanks. While the starting of the plant involved the use of a solution of sulphuric acid for the solvent, all subsequent solvent actions take place in the presence of ferric sulphate and ferrous sulphate as well.

Now, ferric sulphate solutions are energetic solvents of metallic copper, oxides of copper and, to a limited extent, of some sulphides of copper, with reduction from ferric to ferrous solutions and ferric oxide. Take metallic copper and we have this equation:

$$Cu + Fe_2(SO_4)_3 = CuSO_4 + 2FeSO_4.$$

Take cupric oxide:

$$3CuO + Fe_2(SO_4)_3 = 3CuSO_4 + Fe_2O_3.$$

This latter equation shows that ferric oxide (Fe_2O_3) is freed by the solvent action in the leaching tank, and remains there undissolved, thus continually purifying the solution of iron.

The roasting process may sometimes fail completely to oxidize the Cu_2S; if so, then the following equation expresses a further action:

$$Cu_2S + 2Fe_2(SO_4)_3 = 2CuSO_4 + 4FeSO_4 + S.$$

It has been claimed that cupric oxide is dissolved by a solution of ferrous sulphate by double analysis and synthesis, per this equation:

$$CuO + 2FeSO_4 + H_2O = CuSO_4 + (Fe_2O_3 + SO_3) + H_2.$$

The sulphuric acid dissolves some cupric oxide as per this equation:

$$CuO + H_2SO_4 = CuSO_4 + H_2O.$$

It will be seen that all of the reactions result in the production of cupric sulphate in the lixiviate which is to be electrolyzed. There is no deposition of iron on the cathode, even though its sulphate salts are present in the electrolyte in great excess, say near to saturation of the solution.

Unfortunately, the expenses of the operation cannot now be stated, because not yet obtainable. Just as the plant was starting financial difficulties intervened and caused a stoppage. Enough had been done, however, in an experimental way, to make positive all of the principles of the process; and that, probably, the expenses of mining and metallurgical treatment need not exceed two and one-half dollars per ton. It is probable that work will soon be resumed.

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COLLOIDS.

BY W. R. WHITNEY.

The purpose of this paper is to bring before the INSTITUTE the present condition of investigations in a field which, while it is perhaps primarily of chemical interest, promises to be also of physical, electro-chemical and biological interest. It deals with the class of substances known as colloids. The name is due to Graham, who first recognized in gelatinous solutions, such as silicic acid, etc., certain characteristic properties.

It is a well-recognized fact that when two different substances are brought into contact they assume what we call equal and opposite electrostatic charges. This seems to be the case no matter of what the two different substances consist, and is apparently true in case of the solid, liquid or gaseous state. The familiar experiment of rubbing glass or sealing wax with silk, wool or fur is an illustration of this fact, and the action of the various static electric machines depend upon it. The interesting application of the principle in the case of electrostatic differences produced by metals in contact is still a ground for much speculation, and it seems that only in the case of metals which are in contact with solutions, or of different solutions in contact with one another, our knowledge is very far advanced. It should be mentioned, however, that measurements of potential differences between solutions and gases have also been made. We may accept, then, as a postulate, that a physical surface or boundary between dissimilar substances is always the seat of a potential difference.

If a mass of insoluble matter, as quartz, sulphur, turpentine, or even gas bubbles, be suspended in water, the former becomes usually negatively charged, the latter positively. If this material

be finely divided by any mechanical process, as by grinding, no reason appears why it should lose its property of being thus charged, and we are immediately brought to consider a condition which probably exists very commonly in nature, though it has not been generally recognized as such. This has been called the colloidal condition, and it is a condition the characteristics of which may be due to these charges, or, more likely, due to the same cause as are these charges. If the particles of quartz or other material were sufficiently minute, it is probable that the repellent force acting between them (they being all of the same sign) would keep them apart and thus also tend to prevent their settling in water. How this might be finally attained as the particles were reduced in size may be seen when it is considered that their weight would diminish in proportion to the reduction of their volume, while the charge would be constant in potential, but increase with the surface exposed, and, for a given weight, a relatively greater surface is being exposed as the particles are further reduced in size. So that the smaller the particles the greater is the influence of the charges.

On the basis of this consideration we ought not to be surprised to find suspensions in water of insoluble matter which never deposit by the action of gravity alone. Finely divided platinum, gold and other metals difficult to oxidize may be brought into suspension in various ways in pure water, and these suspensions are practically permanent. Sulphur, selenium, silicic acid, the insoluble hydrates and sulphides of metals, etc., may be similarly made to assume a stable state of suspension. In short, a careful consideration of the ways in which such suspensions are actually made, forces one to the conclusion that whenever any substance which is by nature insoluble in water is produced in the water in a very fine state of division, a permanent suspension or so-called colloidal solution will result if electrolytes be absent. This fine state of subdivision should exist (as it probably does) in all cases where the insoluble substance is produced chemically *in situ*, for it is produced molecule by molecule. It should also exist where vapors condense in the water, as occurs with subaqueous electric arcs between metal electrodes.

The question naturally arises at this point, why, then, are such suspensions not identical with solutions? This has been the subject of much study and experiment, and it seems now that we are justified in distinguishing between solutions proper and the

so-called colloidal solutions (or, more correctly, suspensions) by the following characteristics:

A saturated solution, at any temperature, may be made by bringing the solvent and substance to be dissolved into contact, whereby the process of solution usually commences immediately and proceeds to saturation with a velocity dependent upon the surface of substance exposed. This velocity varies at any instant inversely as the degree of saturation then reached. Suspensions or colloidal solutions, on the other hand, are not made in this way. For example, a mass of sulphur brought into contact with water never produces a colloidal sulphur solution.

Again, in a solution proper, the presence of the dissolved substance produces certain effects called osmotic, which are not produced in the case of suspensions. For example, the solution of any non-volatile substance in water will cause a certain rise in the boiling point of the water and the water will also no longer freeze at 32° F., but at a certain lower temperature. This is not the case with the colloidal solutions, for here the presence of the suspended matter is of little or no influence upon the properties of the water. The tendency of the material of suspensions to diffuse in the liquid is very slight or almost absent, while diffusion of material in ordinary solutions is marked, and its tendency or diffusive force, as measured by the osmotic pressure, is very great.

Another difference must be mentioned, and it is this which is of the most interest from the electrochemical or electrical standpoint. *Suspensions* are usually completely coagulated or precipitated by the presence of small quantities of electrolytes, and this does not occur with *solutions*. A suspension of platinum in exceedingly pure water is a black liquid, transparent in thin sections and perfectly stable. The slightest dissolved impurity in most cases will cause its precipitation, if that impurity be an electrolyte, as a salt. Non-electrolytes, as sugar, are without this effect. The quantitative relations which exist between the amount of platinum thus precipitated or coagulated and the electrolyte precipitating it, are such that a simple chemical reaction seems out of the question. The impurities in ordinary distilled water are usually great enough to precipitate this colloid in a few hours, and therefore the permanent suspension can be made only with the greatest care. It is also true, if not in all, at least in most cases, that the colloidal solution or suspension shows its lack of homegeneity in its action on a ray of light. The ray is scat-

tered by the suspended particles and becomes visible in its path through the liquid, the reflected rays being polarized.

It seems probable that through investigations of the properties of the simple inorganic colloids, as for example pure metal suspensions, sulphides or oxides, we may learn something about the heretofore little understood colloids of nature. The importance of this point is seen when it is recognized that most of the reactions of plant and animal growth and life involve colloidal solutions, such as starch, protein matter, ferments, etc.

We have, then, two kinds of solutions. First, the real solution such as filterable, transparent, homogeneous mixtures of acids, bases, salts, sugar, alcohol, etc., with water, where common, well-recognized laws govern the properties; and second, the colloidal solution or suspension wihch is also filterable and transparent, but does not show many other properties characteristic of the real solution. In this latter class are probably most of the reagents used in the mysterious reactions of living organisms.

In the first class are all the mixtures which conduct the electric current and those of sugar, glycerine, etc., which do not; while in the second class there are no electrical conductors. When, however, a pair of charged electrodes are placed in a pure aqueous suspension, or colloidal solution, the colloidal material very slowly moves from the vicinity of the one electrode to the other—some of the colloids moving with, and some against, the positive current. This fact evidently involves a slight conduction, although I have called these bodies non-conductors because this conduction has never been measured and is doubtless very slight. This motion of the colloidal material is another point of evidence favoring the hypothesis of electrostatic charges on the particles, although the simpler assumption of a polarized form of energy is more exact.

Recent work of Coehn and others makes it seem probable that of any two substances in contact, the one with the higher dielectric constant will be positive toward the other.

The theory of electrolytic dissociation is doubtless more or less familiar to most of you, but it is necessary to consider it a moment in connection with this subject. Experiments show that in most simple aqueous salt solutions, a part of the salt is dissociated. The various properties of those solutions which depend on the relative number of particles or molecules present, such as the boiling point, freezing point, etc., are more pronouncedthan is to be expected from the possible number of whole

molecules present. It is also only in the case of these dissociated substances that electrical conductivity through the solution is possible. Moreover, in dilute solutions the degree of this conductivity corresponds to the degree of dissociation as measured by the physical effects referred to. The electromotive force produced by contact of differently concentrated solutions of the same electrolyte may be successfully calculated on the basis of this assumed dissociation and the imagined condition of dissociation seems most probable. The work of Arrhenius, Nernst and others has led to fairly general acceptance of their hypothesis that these dissociated particles are electrostatically charged, and under such an assumption many of the characteristics of these dissociated solutions might be expected. I do not wish to discuss this theory but to call attention to the fact that it is this class of dissociated substances which for some reason exert the most remarkable influence on our colloids or suspensions. The facts will stand alone without the theory and will remain facts, no matter what is the future history of the theory.

A considerable quantity of a non-electrolyte may usually be added to a suspension or colloidal solution without producing any visible effect, while little more than a trace of an electrolyte is often fatal to the stability of such a suspension. Most, if not all, colloidal solutions are thus coagulable by electrolytes.

It has long been known that the cloudy waters often obtained from stamp mills in mining operations and which carry valuable metal, can be made to settle by the addition of various salts. Lime is usually used for this purpose. It has also been recognized that the silt suspended in a river's water, as our Mississippi, is caused to settle at the deltas, not by reduction in current, but by the presence of salts from the ocean. This silt can be precipitated in a bottle by simply adding a little salt solution. Suspensions or colloidal solutions of metals like silver, gold and platinum are readily precipitated by such small quantities of salts, or of electrolytes in general, that a simple chemical reaction is practically out of the question. Many of the dye-stuffs give solutions which act exactly in the same manner. A number of other organic products of the laboratory have this property, and it is also the property of such suspensions as the insoluble metallic sulphides and oxides; of suspensions made by adding alcoholic solutions of gums or resins to water; or by heating to boiling a dilute solution of egg albumen. These are all cases of electrolytic coagulation or precipitation and seem to be representative types

of a large class of processes which occur not only in the chemist's but also in the world's laboratory.

The laws which are being found to exist in the case of these simple suspensions must sooner or later be applied to those cases in Nature where colloidal solutions are undergoing coagulation. It is not absurd to seek their applicability in the case of deposition of starch in the growing plant, of bone and muscle in the maturing animal, etc., for these may well be deposited from colloidal solution by the same actions.

It seems probable from the work already done in this part of the field by various investigators that a small part of the electrolyte which causes the precipitation or coagulation of the recognized suspension is always carried with it into the precipitate. This is usually very small, but a further knowledge of its magnitude in different cases is very much to be desired. Does such an attendant phenomenon occur in the precipitations of nature referred to? The presence of inorganic matter in all of nature's coagulated colloidal material justifies such an assumption of analogy. Nowhere do we find nature's organic products "ash-free," as would be the case if they were the pure colloid.

What function does the electrolyte play in this coagulation? Here speculation is rife. Thus far there seems to be evidence that the coagulation corresponds to the conception of a neutralization of the charges of the suspended particles by oppositely charged parts of the electrolyte.

Many of the facts thus far obtained as the result of experiment can be made to seem consistent on the assumption of already existing hypotheses, and the value of any co-ordination of these facts by these hypotheses must serve as the excuse for their use. We are forced to recognize that the presence of the electrolyte causes the coagulation or precipitation. Bredig assumes that this is brought about by the stress in the dielectric (*i. e.*, the solvent) caused by the presence of the electrostatically charged ions. This would be a natural consequence if the ions are thus charged. Other investigators assume that the electrostatic charges of the particles of the suspension are neutralized by oppositely charged ions of the electrolyte. These charges once neutralized, the forces keeping the particles apart have disappeared and precipitation occurs. This latter seems to accord with many facts, and is a working hypothesis.

Whether a colloid is made up of positively or negatively charged particles may be learned by observing the direction of

its motion between electrodes. It has been found that the coagulating power of a certain salt on a given suspension of a negative colloid is practically a function of the concentration of the positive ion only, and, furthermore, that this ion is retained, in some cases at least, by the precipitated colloid, while the negative ion is not. The precipitating power seems practically independent of the nature of the positive ion except that it varies with its valence. There is only one way in which it seems at all possible that this can occur; namely, that the precipitate produced shall contain the hydrate of the metal ion, and the remaining or clarified solution be correspondingly acid. This has been found to be the case in the single colloid which has been thus quantitatively examined.

Furthermore, the coagulating powers of different electrolytes, as, for example, potassium, barium or ferric chloride, where the valence of the metal differs, and where, according to the hypothesis of statically charged oins, the charges differ, are in that numerical relationship which might be expected from the assumption of a neutralization of charges of opposite sign in the coagulation. This calculation was carried out by Whetham practically as follows:

Evidently equal charges should be available from $2n$ triad ions, $3n$ diads and $6n$ monads, where n is a whole number. Probability of contact between colloid and ion should be proportional to the concentration C of the ion or equal to AC, where A is a constant. The probability of two such ions being together is $\overline{AC}^2$ and of "n" ions $\overline{AC}^n$. For equal coagulative power of ions of different valences $\overline{AC_3}^{2n} = \overline{AC_2}^{3n} = \overline{AC_1}^{6n} = B$ (a constant). The concentration of trivalent, bivalent and univalent ions respectively being C_3, C_2, C_1, hence $C_1 : C_2 : C_3 : = I : B^{\frac{1}{6}} : B^{\frac{1}{3}} = I : K : K^2$, where K is a constant. That is, from the known coagulating powers of univalent and bivalent ions, that of ions of higher valencies could be calculated. Experimental determinations of relative coagulating power for potassium, barium and aluminium have given the ratio of 1: 32: 1020, approximately, which agrees with the calculation.

This means that a given negative colloid will be equally affected or coagulated by solutions of sodium, potassium and ammonium salts of molecularly equivalent concentration, while barium, calcium, strontium, etc., or bivalent elements, will be

much more active coagulants, approximately equal among themselves; and trivalent elements, such as iron, chromium and aluminium, will be most powerful. The ratios of the coagulative powers of these groups, as found by experiment, namely: 1: 30: 1000, also seems to preclude the possibility of simple chemical reactions.

While it is true that most insoluble matter is negatively charged as compared with water, there are some suspensions which are positively charged, and here it is found that the concentration of the negative ion of the electrolyte, instead of the positive, is the factor determining the coagulation. In other words, if chlorine be the ion under consideration, the precipitating powers of all chlorides on this positive colloid will be the same when the concentration of the dissociated chlorine is the same.

If colloidal solutions or suspensions are made up of statically charged particles, the fact that they are precipitated by electrolytes, together with the quantitative relationships of this precipitation and the seeming probability that a part of the electrolyte is rendered insoluble in the process, make the connection of this phenomenon with the electrolytic dissociation theory a most natural one. I personally feel that, conversely, speculation as to possible connections between electrical or electrolytic phenomena in the living plant or animal and the little-understood reactions occurring there is well warranted. Such hypotheses as have been advanced by Professors Loeb and Mathews of Chicago University to explain, among other things, the actions of the nerves and the relationship between nerve phenomena and electrical excitation, certainly lead to renewed attack on old problems and the temporary harm of such a conception even if later shown to be mechanically imperfect, is overbalanced by the numberless additional facts brought to light.

Massachusetts Institute of Technology.

A paper read at the 150th Meeting of the American Institute of Electrical Engineers, New York and Chicago, January 3rd, 1902.

SOME COMMON FAULTS IN EXTERIOR ILLUMINATION.

BY S. EVERETT DOANE.

General illumination may conveniently be considered from two points of view:

First—The actual distribution of light to the object illuminated.

Second—The value of this illumination to the observer.

We can accurately express the value of the first, but can speak only in general terms of the second.

Much has been said about the first and but little about the second, yet the second is the practical end for which all illumination is designed.

Whether or not the source of light is in range of the observer's vision largely influences the second value.

The contrast between the source of light and its environment is all important. A farmer's barn lantern will completely blind one to the road beyond when the night is black, and the vicinity not otherwise illuminated, whereas an arc lamp backed by a white building may be a fairly satisfactory source of light.

I had occasion during many of the evenings last summer to ride over the country roads near my home, and I had plenty of opportunity to notice their illumination. It was my observation that the best lighted towns in my vicinity used kerosene lamps situated at the side of the roads among the tree trunks.

Many and many a time when riding toward an arc lamp have I kept my place on a highway by guiding myself by the shining rail of the street railway track, absolutely unable to tell from the roadbed of shadows what was before me. Actual observations of this character make it clear that the amount of light actually delivered to a roadbed is only a part of what we must consider.

If the source of light must from its character be so placed that the observer is blinded to the road, it avails little as to the candle-power of that source.

An arc lamp usually must be so placed that its light may shine in one's face in order that the light may shine along the road.

Light delivered from a lamp near at hand is much to be preferred to light delivered from a lamp so far away that the rays of light reach the ground at a small angle. Such light magnifies every hummock and equally shadows insignificant hollows and gulleys deep enough to be dangerous.

It would be better to have half the illumination and be able to see the bottoms of the hollow spots.

One of the recent papers on arc lamps spoke of their value in illuminating the houses and trees, and presumably the landscape, where there are no houses and trees.

The interest in this observation lies in the thought that the greater the candle power, and the more infrequent the lamps, the smaller is the portion of the total light shed upon the ground which actually reaches the roadway.

The Massachusetts State roads are uniformly 20 ft. wide. The country roads rarely exceed 30 ft. in width. City roads of Massachusetts are required to be 40 ft.

Arc lamps, located respectively 200 ft.,300 ft. and 400 ft. apart, are evidently intended to illuminate plane areas about 31,500, 70,000 and 125,000 square ft. in extent.

A 20 ft. road crossing this area will receive about 12.7 per cent., 8.6 per cent. and 6.4 per cent. respectively of the light delivered to the ground.

A 40 ft. road will receive twice this percentage, of course, but as the 20 ft. width is all the average rider ever notices, the illustration will answer.

From this it is very apparent why it is wise that lamps of reduced candle power be placed closely together.

From this same illumination it will be seen that lamps placed 200 ft. apart, consuming one-half the energy, may be of one-half the efficiency and still give equally efficient illumination and, what is more to the point, will deliver their light from a more advantageous direction.

One of the best illustrations of suburban street lighting by arc lamps which comes to my mind is on the Boulevard from Boston to Auburndale. The Boulevard is really two separate roads with a continuous narrow park between and the arc lamps are in

the middle of this park, so that they may be said to be in a field adjoining the roads.

Why, with all these draw-backs, are arc lights used almost universally and incandescent lamps to so limited an extent?

I think there will be no discussion as to the reason. Incandescent lamps for street lighting have always been difficult to make and less satisfactory than any other type of incandescent lamp.

The ordinary 100 volt to 125 volt 16 c.p. 3 l.w.p.c. incandescent lamp gives more candle hours for a given input than any other incandescent lamp. If this lamp could be used for street lighting the cost of street lighting by incandescent lamps could be materially reduced and the service rendered be much more satisfactory to the user of the highway.

There are many streets in which low voltage mains run the length of the streets and where these lamps could be used in multiple with no inconvenience. There are many other streets where ten or twenty could be used in series, each on a cut-out box, no more complicated than the Nernst lamp, which you will possibly be called upon to consider. These cut-out boxes would not be so complicated as the arc lamps, which operate with practically no attention.

I do not understand why this has not been done.

Times have changed. The arc lamp lines are no longer the only wires on the poles. Probably one-half of the electric lighting poles carry alternating current mains also.

Incandescent lamps are now universally fastened to their bases by a waterproof cement, and waterproof sockets are in stock everywhere.

Is it not time that we broke away from tradition?

The arc lamp has its use in street lighting. There are places where it is wisest to use it. The public think they are getting more for their money with the brilliant arc lamp, but I believe that as the users of the highways come to appreciate how much safer and more satisfactory well-placed illumination is than that to which I have referred, we will learn to light our streets as we do our homes.

The lesson learned in show-window lighting and in theatre stage lighting should not be lost upon us.

It is not the light, after all, that interests us; it is what we see by means of the light.

We must add to this that for a given expenditure of energy more light can actually be delivered to a highway by incandes-

cent lamps than by arc lamps and it can be better distributed.

Arc lamps are at their best in densely settled districts. The reflected light from buildings helps to economize the distribution and the great quantity of light from store windows also assists so to brighten the surroundings that the brilliant arc lamp is less blinding. Arc lamps are also more closely spaced in these localities, which is to their advantage.

Incandescent lamps are, in my opinion, much to be preferred for residential streets, side streets and outside the more congested portions of a town or city.

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

NEW YORK, Jan. 24. 1902.

The meeting was called to order by President Steinmetz at 8.15 P.M.

SECRETARY POPE:—At the meeting of the Board of Directors this afternoon the following Associates were elected:

NAME.	ADDRESS.	ENDORSED BY
ADAMS, WILLIS LONGFELLOW,	General Manager, Adams Construction Co., 726 Buffalo Ave., Niagara Falls., N.Y.	W. H. Ripley. C. Van Deventer. J. F. Kelly.
ARNOLD, WARD S.	Electrical Engineer, Railway Department General Electric Co., Monadnock Building; residence 384 E. 40th St., Chicago, Ill.	James Lyman. H. E. Chubbuck. G. W. Knox.
BINDEMANN, HARRY	OTTO FERDINAND, Electrical Engineer, Union Electricitats Gesellschaft, 43 Dorotheen Str., Berlin, Germany.	H. M. Hobart. W. S. Hulse. R. W. Pope.
BLACKWELL, HOWARD	LANE, Assistant in Physics, Harvard University, 13 Little's Block, Cambridge, Mass., residence East Orange N. J.	C. A. Adams, Jr. S. S. Wheeler. G. S. Dunn.
CABOT, SEWELL,	Manager, Back Bay Exchange, New England Tel. and Tel. Co., 208 Newbury St., Boston; residence, High Street, Brookline, Mass.	H. W. Wilder. F. L. Gilman. G. F. Atwood.
CALDWELL, EUGENE	WILSON, Director, Edward N. Gibbs Laboratory, University and Bellevue Hospital Medical College, 315 Fifth Ave.; residence, 20 E. 31st St., New York City.	W. D. Weaver. C. T. Child. J. J. Carty.

DIXON, JAMES,	Electrical Engineer, Gray National Telautograph Co., Room 142, 80 Broadway, New York City; residence, 241 Franklin Place, Flushing, N. Y.	E. F. Peck. Geo. F. Sever. F. B. Crocker.
EHRET, CORNELIUS DALZELL,	Examiner, U. S. Patent Office, Room 9, Washington, D. C.	G. K. Woodworth E. E. Clement. C. L. Sturtevant.
FLEMING, RICHARD,	Designing Engineer, General Electric Co., Lynn, residence, Swampscott, Mass.	C. P. Steinmetz. W. C. Fish. A. L. Rohrer.
GRAHAM, WILLIAM P.,	Professor of Electrical Engineering, Syracuse University, 504 University Place, Syracuse, N. Y.	E. L. Nichols. W. B. Hodge. J. G. Biddle.
JONES, FRED ATWOOD,	Railway Engineering Department, General Electric Co., Schenectady, N. Y.	H. E. Mole. H. J. Ryan. W. I. Slichter.
KIER, SAMUEL MARTIN,	Electrical Engineering Dept., Westinghouse E. & M. Co.; residence, 5904 Elgin Ave., Pittsburg, Pa.	C. F. Scott. R. A. Fessenden. N. W. Storer.
LILLIBRIDGE, RAY D.,	Stanley Electric Mfg. Co., 20 Broad St., New York City; residence, 146 Quincy St., Brooklyn, N. Y.	T. C. Martin. W. D. Weaver. F. A. C. Perrine.
MORGAN, CARL LEON,	Assistant to Purchasing Agent, Edison Electric Illuminating Co., 3 Head Place; residence, 25 St. Cecilia St., Boston, Mass.	C. L. Edgar. C. H. Harris. Joseph Sachs.
PARSONS, ISAAC DAYTON,	Assistant in Electrical Engineering Dept., Columbia University; residence, 314 W. 23d St., New York City.	F. B. Crocker. G. F. Sever. F. Townsend.
RANDOLPH, MERVYN PAUL,	District Office Manager, Westinghouse E. & M. Co., 314 Occidental Ave., Seattle, Wash.	C. F. Scott. R. S. Masson. E. M. Tingley.
RYAN, WALTER D'ARCY,	Engineer, General Electric Co., Lynn, Mass.	E. Thomson. W. C. Fish. C. P. Steinmetz.
SEABROOK, HENRY HAMILTON,	Sales Engineer, Westinghouse E. & M. Co.; residence, 822 Connecticut Ave., Washington, D. C.	C. F. Scott. P. H. Lange. W. K. Dunlap.
SULLIVAN, WILLIAM VAN AMBERG, JR.,	2d Assistant Engineer, U. S. S. Rush, Sitka, Alaska.	A. G. Davis. G. K. Woodworth. T. W. Ross.
TERRY, ALBERT SLOCOMB,	Treas. and Manager, The Sunbeam Incandescent Lamp Co., 463 West St.; residence, 59 Morningside Ave., New York City.	G. F. Atwood. G. A. Hamilton. P. G. Burton.
TOURET, MAXIME EUGENE JEAN,	Electrical Consulting Engineer, 7 Rue Meyerbeer, Paris, France.	Chas. Le Blanc. R. W. Pope. W. J. Hammer.

VON RECKLINGHAUSEN, MAX,	With Mr. George Westinghouse, 510 West 23d St.; residence, 11 East 24th St., New York City.	C. A. Terry. G. H. Stockbridge A. J. Wurts.
WARREN, HENRY ELLIS,	Assistant Engineer, The Lombard-Governor Co., Newton Centre, Mass.	A. V. Garratt. Chas. Cross. R. W. Pope.
WHITESIDE, WALTER HUNTER,	Manager, Westinghouse E. & M. Co., 1317 F. St.; residence, 1405 W. St., Washington, D. C.	C. F. Scott. L. A. Osborne. C. E. Skinner.
YAMAZAKI, SHIRO,	Student of Electrical Engineering, Cornell University, 408 University, Ave., Ithaca, N. Y.; residence, Tokyo, Japan.	E. L. Nichols. H. J. Ryan. A. L. Hoxie.

Total, 25.

PRESIDENT STEINMETZ.—Gentlemen: I have to announce that the next meeting of the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS will be a special meeting, to be held on the evening of Feb. 14 at 8 P.M., at Columbia University. At this meeting a paper will be read by Mr. Percy Thomas on static strains in high tension circuits and the protection of apparatus. This paper will be illustrated by a number of very interesting experiments and therefore the lecture will be given at Columbia University. Ladies are invited.

The topic for to-night's meeting is the electric railway. While electric railroading is perhaps the most important branch of electrical engineering, at least as regards commerical importance, considering the amount of capital invested therein, nevertheless it is a remarkable fact that while most other branches of electrical engineering had been developed to a very high degree of perfection, even a few years ago theoretical investigation of electric railroading was still conspicuous by its almost entire absence.

All the work was done by some kind of empirical experimenting, that is, some motor was fitted up with some gearing or some sort of railway car, and then run, and if the motor burned out frequently it was replaced by a larger motor, and if it did not burn out, a trailer was put on the car, and perhaps a second trailer, until the increase of the expense account in burn-outs of the motors balanced the increased carrying capacity of the train.

Perhaps the first theoretical investigations of electric railway problems are contained in a number of papers presented to the INSTITUTE on train acceleration by one of my former assistants, Mr. A. H. Armstrong. They led to the proposition of the typical motor cycles, the type curve in Fig. 1 of Dr. Hutchinson's paper. That is a curve with time as abscissa and velocity as ordinate, consisting of acceleration, coasting and retardation or braking.

One of the most interesting results brought out by this investigation was the enormous importance, in its bearing on the motor capacity, of the number of stops and the frequency of

stops. It followed that by increasing the number of stops per car mile, with the same schedule speed, the motor capacity had to be increased very greatly or else with the same motor capacity, the same number of stops rapidly reduced the permissible schedule speed. Some apparently paradoxical results were arrived at also, as for instance, the greater the power input into the motor in starting, the less is the total energy required to carry the railroad car at a given time over a given distance; and the less the maximum speed attained during the run, the greater is the amount of energy required to carry the car over a given distance in a given time.

These results at first were based on the type curve on Fig. 1 of Dr. Hutchinson's paper. By considering the motor curve, that is, the acceleration of the railway motor after the rheostat has been cut out, due to the decreased magnetization caused by a decrease of current, and corresponding decrease of motor torque, those results derived were and are modified in a more or less essential degree. From our present point of view, with our present knowledge, all of these results are really self-evident—as self-evident as anything is as soon as you know it.

The problem is to carry a given weight in a given time over a given distance. To do that the motor has to overcome the mechanical friction, which is, more or less approximately, equal to train weight multiplied by coefficient of friction multiplied by distance. It is fairly independent of the character of motor control and also of the motor speed, within certain limits. This, then, is the work which is absolutely required for the carrying of the car over the track and therefore may be considered as the useful motor work. In addition thereto the motor has to give also the mechanical work which in stopping is consumed and annihilated by the brakes; that is, the mechanical output of the motor is greater than the useful work by the amount of energy consumed by the brakes, and the ratio of useful work, that is, energy consumed by train friction to total mechanical work of motor, that is, train friction plus energy consumed by the brakes, is what may be called the mechanical efficiency of the motor cycle.

Now, we immediately distinguish two different types of railway work here. One of them is the long-distance run, with very few stops, in which the energy consumed by the brakes in stopping is insignificant compared with the mechanical output of the motor. The mechanical efficiency is practically unity (except when descending heavy grades on the brakes), and therefore the rate of acceleration, retardation, etc., is immaterial. The other is when the railway motor is operated with frequent stops and short distances, as in city and rapid transit work, in which the energy consumed by the brakes is a considerable part of the mechanical output of the motor. There the mechanical efficiency has to be considered.

Now, it is obvious that the maximum mechanical efficiency is reached where the amount of energy consumed by the brakes

in stopping is a minimum; that is, when the brakes are applied at the minimum possible speed, since the energy consumed by the brakes is proportional to the square of the velocity at the moment of applying them. Under given conditions of distance and schedule speed, it follows herefrom immediately that for mechanical efficiency the most efficient operation of the motor is to accelerate and to retard as quickly as possible, at as high a rate as possible, and to coast in between. The total efficiency of the motor cycle, however, as the ratio of useful work to total electrical input in the motor, is the product of the mechanical efficiency, as discussed above, and the electrical efficiency of the motor, that is the ratio of the mechanical output of the motor to the electrical input.

The operation of the motor during acceleration consists of acceleration on the rheostat and acceleration on the motor curve—the acceleration on the motor curve being at very high efficiency. In the acceleration on the rheostat the mean efficiency necessarily must be below 50%, because, assuming a constant acceleration, the input into the motor is constant, independent of the speed, while the speed varies from zero to a maximum, that is, to the speed where the rheostat is completely cut out. The average speed is half of this speed, while the energy input corresponds to the maximum speed, so that the actual efficiency during the period of acceleration on the rheostat is approximately the efficiency of the moment when the rheostat is completely cut out, divided by two. This is not exact, because at lower speed the core loss is less, but it follows therefrom that the maximum electrical efficiency requires to have as long a range of acceleration on the motor curve and as short a range on the rheostat as possible. During acceleration on the motor curve, the current decreases, that is, the acceleration decreases, and, consequently becomes less than the maximum acceleration assumed, with reference to the maximum discomfort the passengers will stand, etc. The greater the range of acceleration on the motor curve, the higher is the electrical efficiency, but the lower is the mean acceleration, that is, the lower the mechanical efficiency; and you see herefrom that the two components of total efficiency, electrical and mechanical efficiency, are antagonistic, the mechanical efficiency requiring maximum rate of acceleration, the electrical efficiency requiring maximum range of acceleration on the motor curve, that is, lesser mean acceleration. The problem of the designing of the motor cycle in an efficient electric motor control consists then in combining the two efficiencies and the rate of acceleration, etc., in such a manner as to get the maximum resultant efficiency.

After these introductory remarks I call upon Dr. Cary T. Hutchinson to present his paper.

Dr. Cary T. Hutchinson then presented his paper entitled "The Relation of Energy and Motor Capacity to Schedule Speed in the Moving of Trains by Electricity." [See February, 1902, p. 117].

Discussion.

President Steinmetz:—We all agree that Dr. Hutchinson's paper is only an approximation, and some of us think a rough approximation. The designing of railroad equipments five years ago, was all according to judgment and with an accuracy of say two or three hundred per cent. or so. At present, if we get accurate profiles of the road, schedule speeds, number of stops and all the data, and carefully investigate them, considering the characteristic curve of the particular motor which we intend to use, we can get very close, but between these two cases there is something more. We want to get an approximate estimate very much closer than the thumb rule estimate of years gone by, but still an approximation of general character, which does not need to be as close as might be required to fit each individual case, within a few per cent., and that is the thing which I believe the paper to supply. I now call upon Mr. W. C. Gotshall.

Mr. W. C. Gotshall.—Somewhat over a year ago the New York & Port Chester Railroad Company commenced an investigation upon heavy and high-speed electric traction. In the pursuit of this investigation there have been prepared by the engineers of the New York & Port Chester Railroad, and directly under the supervision of and by my associate, Mr. C. O. Mailloux, and myself, several hundred "run-sheets," the object of these investigations being, of course, to determine the economical and proper conditions of operation for the proposed service.

I received the advance copy of Dr. Hutchinson's paper a few days ago. After reading the paper, it was apparent to me that the results and conclusions arrived at by Dr. Hutchinson in this paper were diametrically opposite to those which we had obtained in the study of the technical details of electric train service proposed for the New York & Port Chester Railroad Company.

On account of the difference between the results which we obtained and the conclusions of Dr. Hutchinson's paper, I will here call your attention to a few of the typical run-sheets which have been worked out by us and which are now the property of the New York & Port Chester Railroad Company.

The paper which has been read purports to be, as stated in the first paragraph, a general solution covering all cases which can arise in ordinary practice of train movement. This statement is very broad—certainly broad enough to include, as a particular case, high-speed train movement such as would be required for rapid transit service. It is more particularly this aspect of the paper which I propose to discuss. It will be my purpose to confine this discussion to the fundamental points upon which the treatment of the paper rests, so far as I have been able to grasp them by examination of the curve sheets and by perusal of portions of the text. I may say that I am not quite prepared to look upon this solution as having a character and application quite as far-reaching as the author's statement would imply, for the rea-

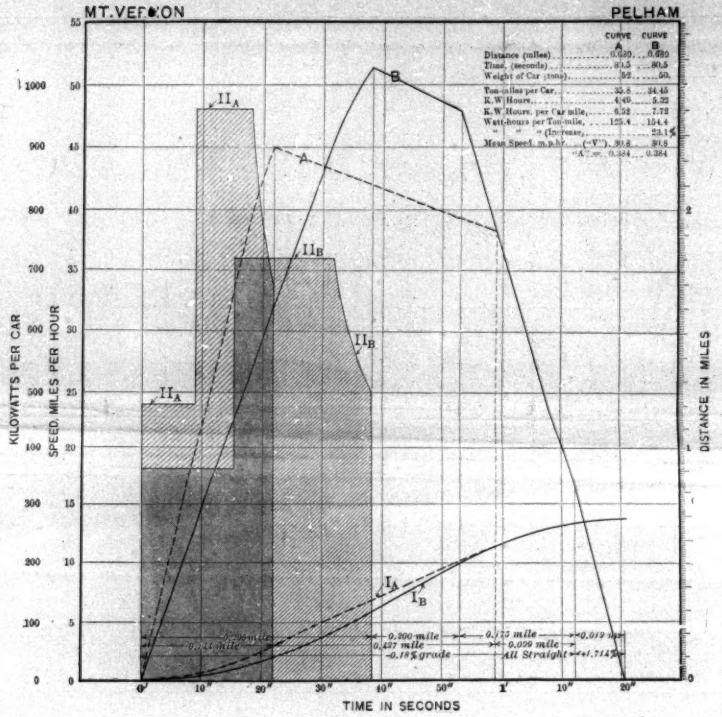

MT. VERNON
PELHAM
KILOWATTS PER CAR
SPEED, MILES PER HOUR
DISTANCE IN MILES
TIME IN SECONDS
CURVE A
CURVE B
Distance (miles) 0.620 0.620
Time, (seconds) 93.5 80.5
Weight of Car (tons) 55 50
Ton-miles per Car 55.8 44.45
K.W. Hours 4.46 3.22
K.W. Hours per Car mile 8.52 7.72
Watt-hours per Ton-mile 125.4 154.4
" " " (Increase) 23.1%
Mean Speed, m.p.hr. (-V'). 30.8 31.9
"A" = 0.354 0.384
II A
II B
III A
III B
I A
I B
A
B
0.200 mile
0.427 mile
-0.18% grade
0.175 mile
0.099 mile
All Straight
0.019 mile
+1.714%
0.02 K.W. Hrs
7.06 K.W. Hrs
RYE NECK
RYE
Level
3° Curve
0.453 mile
0.284 mile
-0.221% grade
0.194
0.363 m.
0.134
0.135 m.
0.132 m.
0.144 m.
0.130 m.
0.118 m.
Distance, (miles) 1.448 1.448
Time, (seconds) 138½ 138½
Weight of Car, (tons) 52 50
Ton-miles per Car 75.3 72.4
K.W. Hours 6.04 7.06
K.W. Hours per ton-mile 5.75
" " " (Increase) 18.4%
Mean Speed, m.p.hr. (-V'). 37.7 37.7
"A" = 278 279

FIG. 1.

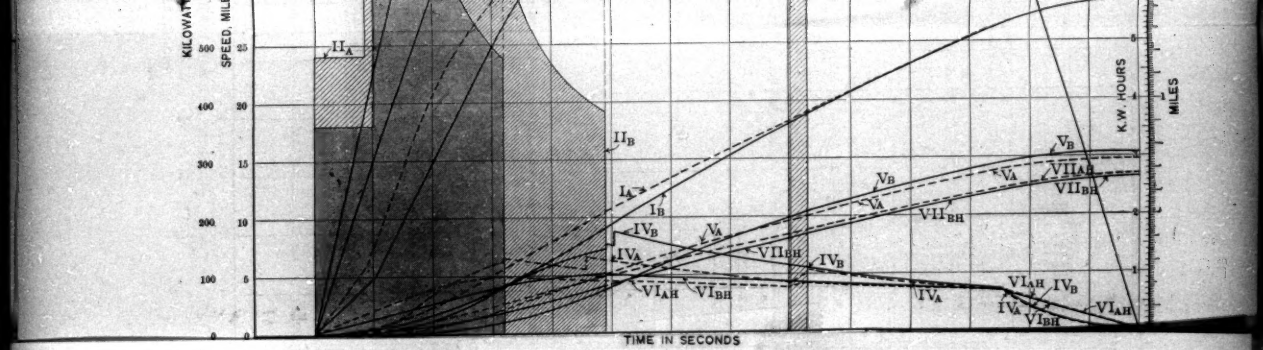

KILOWATTS PER CAR
SPEED, MILES PER HOUR
K.W. HOURS
MILES
TIME IN SECONDS
II_A
II_B
I_A
I_B
IV_B
IV_A
V_A
V_B
VII_BH
VII_AH
VI_AH
VI_BH

FIG. 2.

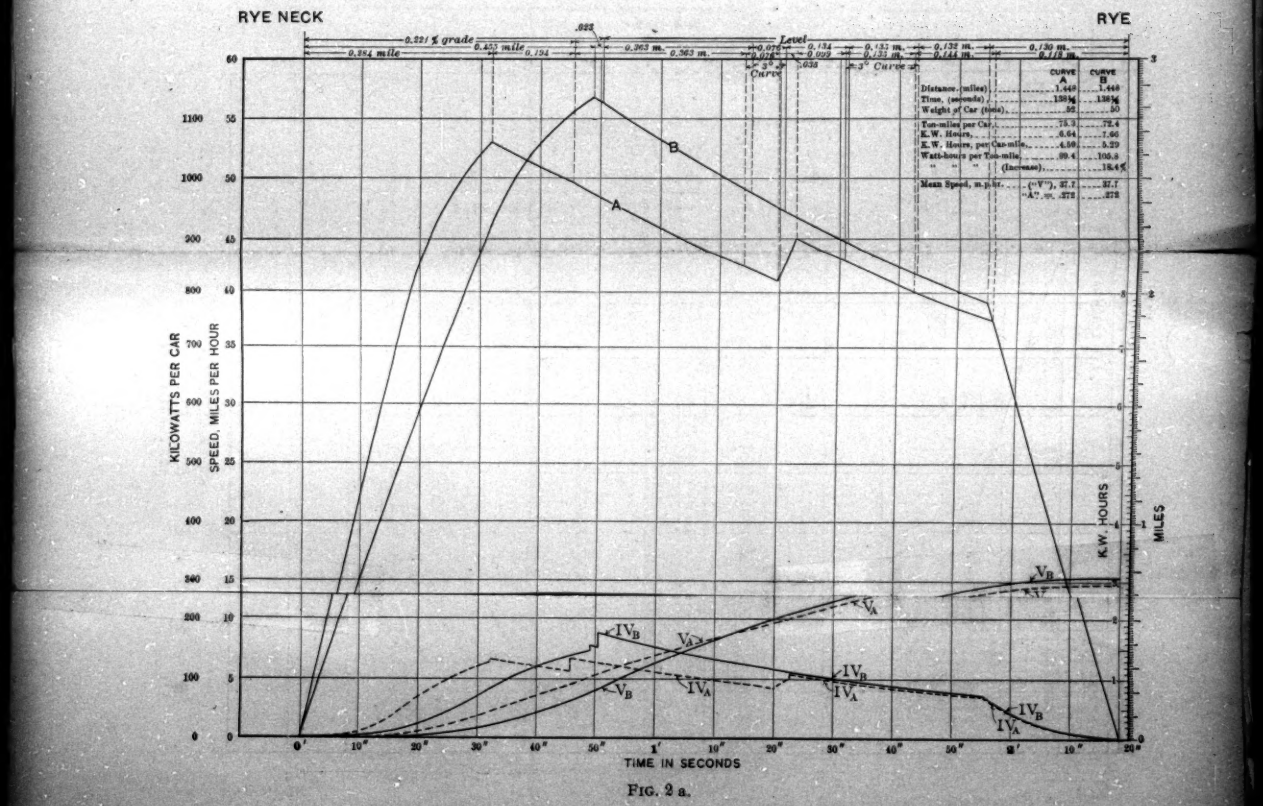

RYE NECK
RYE
0.221 % grade
0.284 mile
0.455 mile
0.194
.022
0.363 m.
0.363 m.
Level
0.076
0.134
0.132 m.
0.132 m.
0.132 m.
0.130 m.
0.078
0.009
0.133 m.
0.144 m.
0.118 m.
3° Curve
.038
3° Curve
CURVE A
CURVE B
Distance, (miles) 1.449 1.449
Time, (seconds) 135½ 135½
Weight of Car (tons) 95 95
Ton-miles per Car 73.3 72.4
K.W. Hours 6.64 7.66
K.W. Hours, per Car-mile 4.59 5.29
Watt-hours per Ton-mile 89.4 105.5
" " " (Increase) 18.4%
Mean Speed, m.p.hr. (=V'), 37.7 37.7
=A'...sec. .372 .372
KILOWATTS PER CAR
SPEED, MILES PER HOUR
K.W. HOURS
MILES
TIME IN SECONDS
A
B
V_B
V_A
IV_B
IV_A

FIG. 2 a.

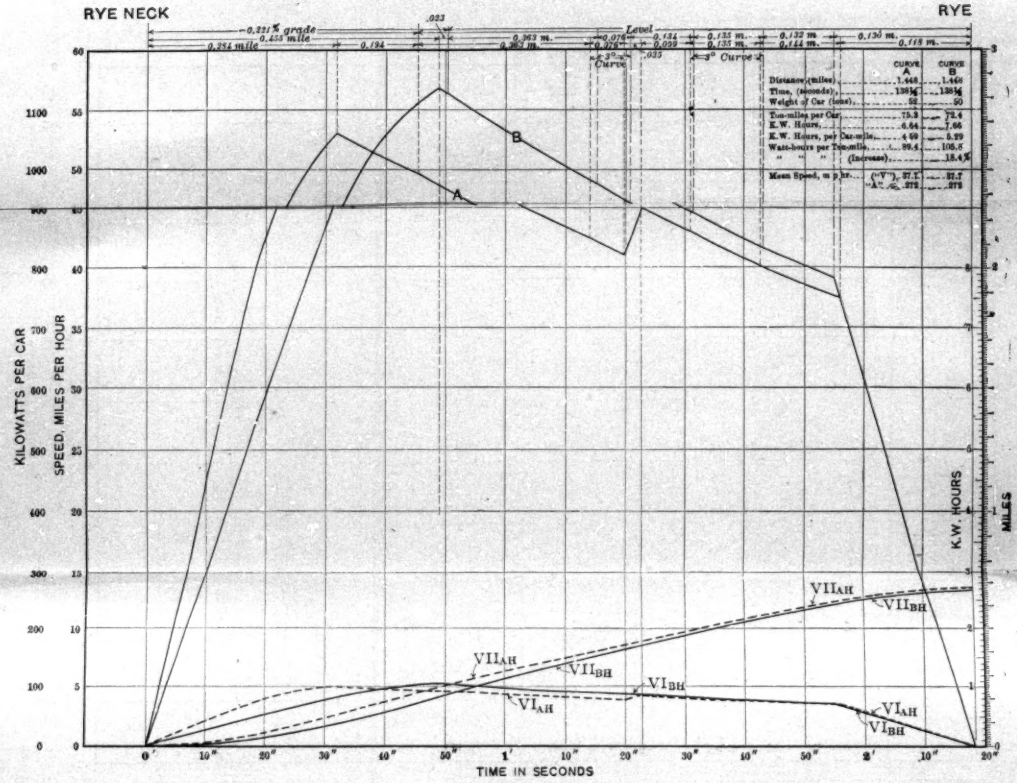
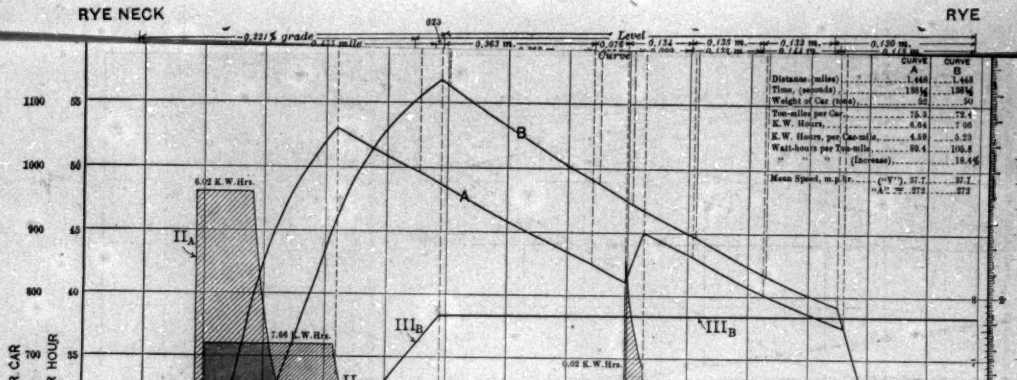

RYE NECK
RYE
0.221% grade
0.254 mile
0.455 mile
0.194
.023
0.363 m.
0.382 m.
Level
0.076
0.074
0.134
0.135 m.
0.132 m.
0.144 m.
0.130 m.
0.118 m.
3° Curve
.035
CURVE A
CURVE B
Distance (miles) 1.448 1.448
Time, (seconds) 138½ 138½
Weight of Car (tons) 52 50
Ton-miles per Car 75.3 72.4
K.W. Hours, 6.64 7.66
K.W. Hours, per Car-mile 4.59 5.29
Watt-hours per Ton-mile 89.4 105.8
" " " (Increase) 18.4%
Mean Speed, m.p.hr. (=V'). 37.7 37.7
=A'C = .272 .272
KILOWATTS PER CAR
SPEED, MILES PER HOUR
K.W. HOURS
MILES
A
B
VII_AH
VII_BH
VI_BH
VI_AH
TIME IN SECONDS

FIG. 2 b.


RYE NECK
RYE
0.221% grade
0.254 mile
0.455 mile
0.194
.023
0.363 m.
0.382 m.
Level
0.076
0.074
0.134
0.135 m.
0.132 m.
0.144 m.
0.130 m.
0.118 m.
3° Curve
.035
CURVE A
CURVE B
Distance (miles) 1.448 1.448
Time, (seconds) 138½ 138½
Weight of Car (tons) 52 50
Ton-miles per Car 75.3 72.4
K.W. Hours, 6.64 7.66
K.W. Hours, per Car-mile 4.59 5.29
Watt-hours per Ton-mile 89.4 105.8
" " " (Increase) 18.4%
Mean Speed, m.p.hr. (=V'). 37.7 37.7
=A'C = .272 .272
6.02 K.W.Hrs.
7.66 K.W.Hrs.
6.02 K.W.Hrs.
II_A
III_B
III_B
A
B
KILOWATTS PER CAR
SPEED, MILES PER HOUR
K.W. HOURS
MILES

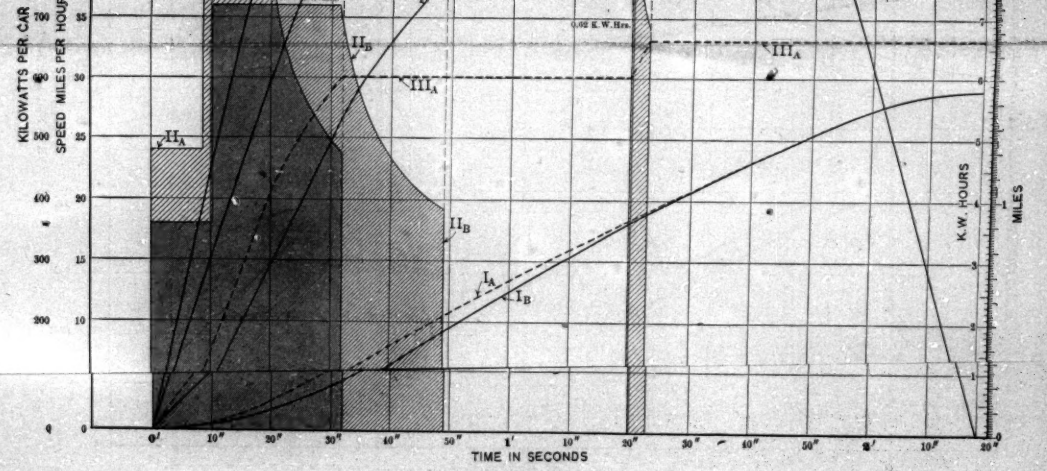


FIG. 2 c.

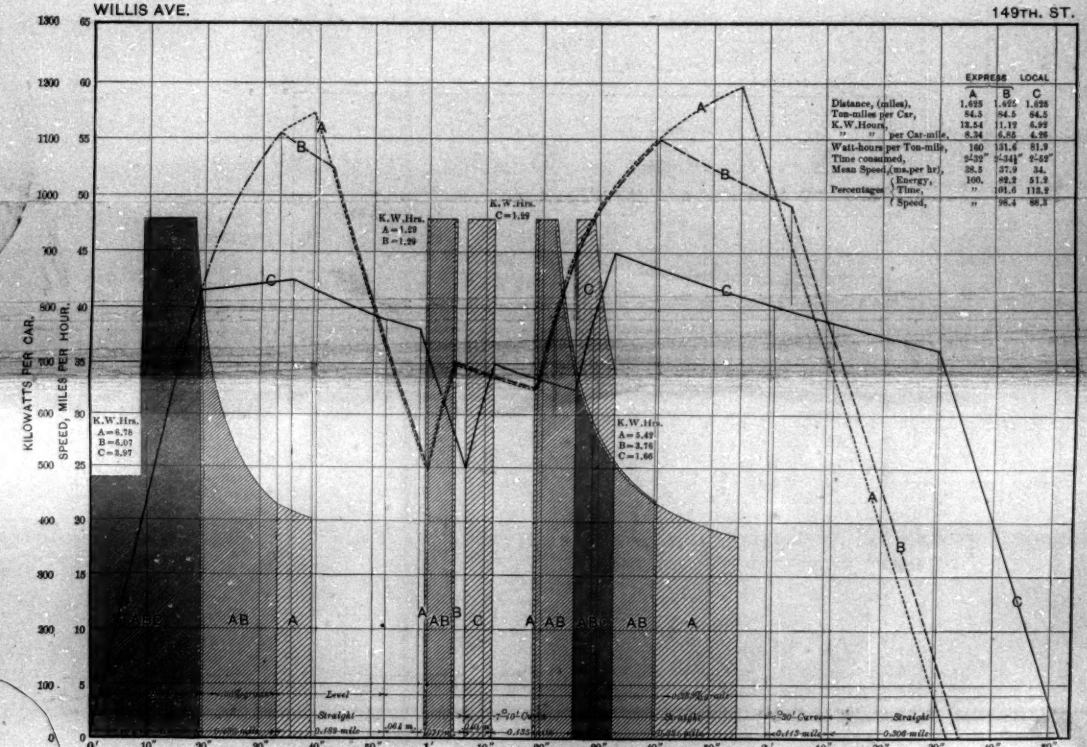


FIG. 3.—Run Sheet from Willis Ave. to 149th Street.

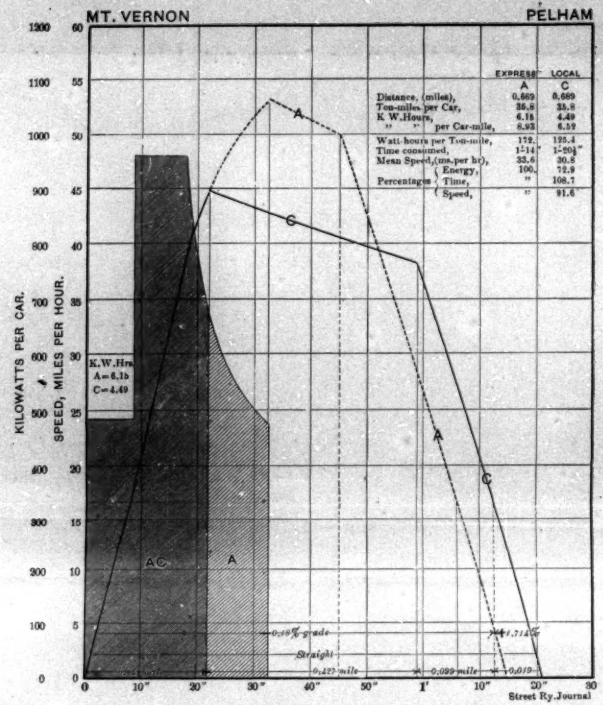
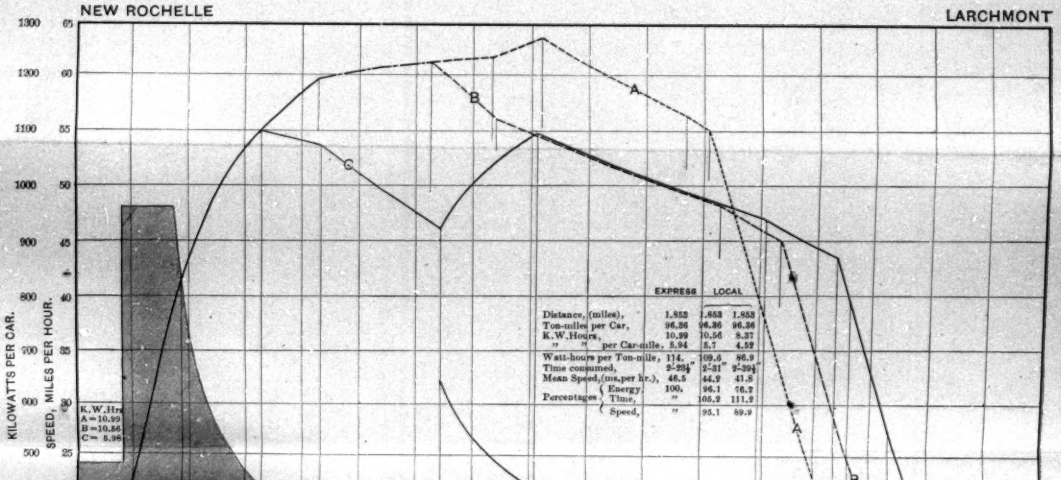


FIG. 4.—Run Sheet from Mt. Vernon to Harlem.



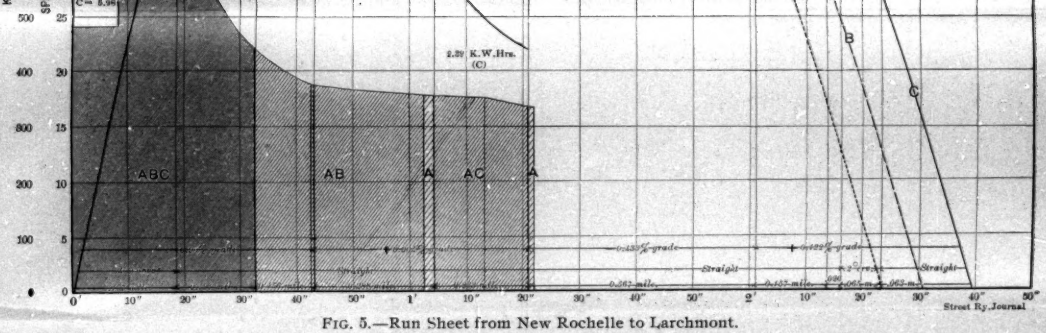


FIG. 5.—Run Sheet from New Rochelle to Larchmont.

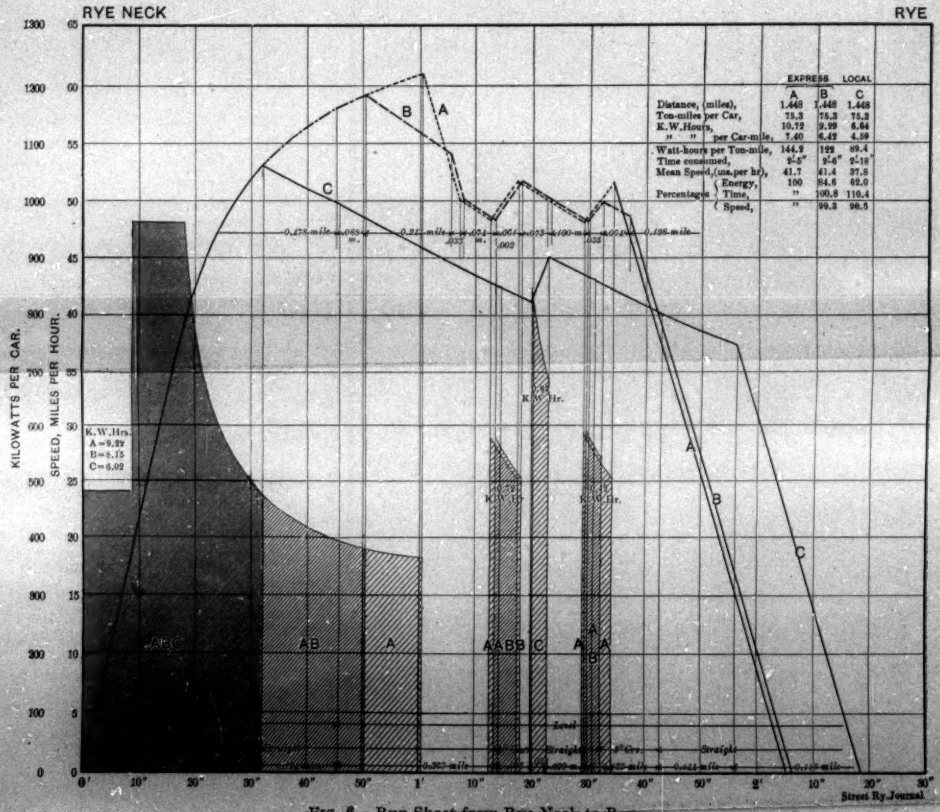


FIG. 6.—Run Sheet from Rye Neck to Rye.

son that it fails to take into consideration some important points which generally involve and determine very important modifications in practical results. We all agree as to the desirability of finding and developing graphical methods for computing and analyzing the various factors and elements involved in the problem of train movement under certain given conditions. I am not prepared to believe, however, that it is wise to attempt a too broad or sweeping generalization by simplification at the expense of accuracy. The simplification is apparently obtained, in this case, by making many of the conditions a function of one principal, independent variable, which is called the "through acceleration" and designated by the letter A. Now, it is true that, in electric railroad work, as in other branches of our profession, in dealing with any problem we must always bear in mind the old adage that circumstances alter cases. These circumstances assume a difference in relative importance in many cases to such an extent that, while they may be of minor consideration in some cases, they become the primary consideration, or, literally, the independent variable, in other cases. Take, for instance, rapid transit work. In this case time is the paramount consideration; economy becomes a secondary one—contrary to the author's conclusion that economy of power is the sole governing consideration in the selection of an equipment. In high-speed work, the speed, in fact, is limited only by the absolute technical and commercial possibilities and impossibilities of the case.

If I interpret the author correctly, there are at least seven assumptions which he has made, apparently to simplify the mathematical treatment of the subject, which assumptions are apt to be fallacious in many cases and to require modification in almost all cases. These assumptions are the following:—

First. That the line be absolutely straight.

Second. That it be absolutely level.

Third. That the train resistance is constant at all speeds.

Fourth. That all runs for the entire length will not differ materially from the average length of run.

Fifth. That the power will be applied during the first part of the run only; that is to say, only on the first part of the speed-time curve.

Sixth. That the general type curve can be used with simple corrections for calculating the energy consumption under all conditions.

Seventh. That the rules and formulas applying to tramcar practice will also apply to rapid transit practice.

I do not think that the conclusions of Dr. Hutchinson, or his method of arriving at those conclusions, are at all applicable to practical conditions. In this paper Dr. Hutchinson has used and applied the letter A as a symbol designating what he chooses to call "through acceleration," which is a purely abstract idea, the physical meaning of which is not easy to realize. For the purpose of ascertaining what range of value this term may have,

I have taken a number of typical runs for the New York & Port Chester Railroad as they have been worked out, and have determined the value for A for each of these runs. These determinations are shown in Table 1 in the column marked " A." The variations for the individual runs in the values of this quantity are very great, the difference being as much as 200 per cent. I believe that Dr. Hutchinson intends the value of " A " to be a sort of average acceleration factor. If such be the case, I call your attention to the fact that it would be impossible to arrive at any value of the average acceleration factor which could be used in practice from the results shown in Table 1. This is due to the material difference in the lengths of runs, as well as to the grades and curves which influence the maximum speed attained. In Table 1, under the column marked "angle," are shown the inclinations of the various acceleration lines. The column marked " coefficient " gives the numerical value of the " tangent " of the corresponding angle, which is equal to the corresponding rate of acceleration in m.p.h. per second. The variation in the value of these angles is undoubtedly of interest, but cannot be a matter of any great surprise. These variations might, indeed, have been expected, since any condition of the line other than a level condition must of necessity produce a different inclination for the line of acceleration, and, consequently, must determine a different value for the angle.

It may be well here to give a summary of the principal data in regard to this project. The project contemplates a four-track line running from the Harlem River to the Connecticut State line at Port Chester. Two of the tracks are to be used for express train traffic and two for local traffic. The total length of the main line, according to the present surveys, is 20.9 miles. The express trains will serve a total of eleven stations, making ten stops—the longest run being 3.114 miles and the shortest run 0.689 mile, and the average length 1.9 miles. The local trains will serve a total of twenty-two stations, making twenty-one stops. The longest local run is 1.853 miles and the shortest run (No. 10) is 0.428 mile, the average being 0.95 mile. The two main diagrams (Figs. 1 and 2) show the speed-time curves for local train runs No. 11 (Fig. 1) and No. 20 (Fig. 2). Some of the curves in Fig. 2 are reproduced separately in Figs. 2a and 2b. Table I gives various data in regard to the express runs. Table II gives certain data showing the influence of grades on the initial acceleration.

The schedule time proposed is thirty-one minutes for express trains and forty-nine minutes for local trains. The schedule speed is, therefore, 40.58 m.p.h. for express trains and 25.6 m.p.h. for local trains. An allowance of fifteen seconds is made for stops. The mean speed, or average velocity, is, therefore, 44 m.p.h. for express trains and 28.9 m.p.h. for local trains.

Each car is to be equipped with four motors, one on each axle, thereby making the entire weight available for traction. The

TABLE I.

CONCERNING EXPRESS RUNS OF NEW YORK & PORT CHESTER RAILROAD.

Run No.	From	To	Distance Miles	Time Seconds	Watt Hours per Ton Mile	Speed			Per Cent. Grade at Starting	Acceleration				
						Highest	Lowest	At Braking		Coeff.	Angle		V	A
											Deg.	Min.		
1	Willis Avenue	149th Street	1.625	152½	160	57.5	25.0	59.7	....	2.15	65	5	38.35	.264
2	149th Street	Bronx Park	2.751	204	141	60.5	40.0	62.7	....	2.15	65	3	48.6	.238
3	Bronx Park	Bronx and Pelham Parkway	1.429	91½	169	60.4	60.4	60.4	+0.65	2.02	63	40	56.0	.614
4	Bronx and Pelham Parkway	Mount Vernon	3.104	218½	100	67.0	57.8	57.8	+0.163	2.12	64	45	51.2	.234
5	Mount Vernon	Pelham	0.689	72	172	53.3	50.0	50.0	—0.18	2.20	65	34	34.5	.479
6	Pelham	New Rochelle	1.843	166	142	54.5	38.4	50.3	+1.714	1.80	61	0	40.0	.241
7	New Rochelle	Larchmont	1.853	144	114	63.4	....	55.0	—0.90	2.36	67	2	46.35	.322
8	Larchmont	Mamaroneck	1.22	113½	158	57.6	42.7	55.0	+0.122	2.11	65	45	38.7	.341
9	Mamaroneck	Rye Neck	2.883	207½	111	62.7	41.7	55.4	—0.261	2.22	65	45	50.1	.2415
10	Rye Neck	Rye	1.45	105½	142	60.8	48.0	51.5	—0.22	2.21	65	39	49.5	.469
11	Rye	Port Chester	2.09	127	159	58.8	36.0	52.2	+0.424	2.26	66	8	59.2	.467

total weight of car for express service, fully loaded, is 52 tons, of which 17 tons is the weight of the complete motor equipment. For the local service, another car has been under consideration having an equipment of four smaller and lighter motors, making the total weight of car 50 tons, with full load.

The diagrams in Figs. 1 and 2, already referred to, have been selected out of a large assortment of similar diagrams relating to various conditions of service and operation because they show a comparison of the energy required for a given run with the two different kinds of motor equipments giving different accelerations. These diagrams have been prepared with the greatest care, every precaution being taken to consider, and to make due allowance for, every factor entering into the case. The data from which these curves were made were collated with the greatest care, and may be said to be substantially free from any assumption or guesswork whatever.

TABLE II.

EFFECT OF GRADES ON INITIAL ACCELERATION.

GRADE Per Cent.	300 AMPERE MOTOR				400 AMPERE MOTOR			
	Speed	Acceleration			Speed	Acceleration		
	10 Seconds After Starting (M. P. H.)	Coeff. (V. T.)	Initial Angle		10 Seconds After Starting (M. P. H.)	Coeff. (V. T.)	Initial Angle	
			Deg.	Min.			Deg.	Min.
Up +2.0	10.2	1.02	45	34	17.4	1.74	60	7
" +1.5	11.3	1.13	48	30	18.4	1.84	61	29
" +1.0	12.4	1.24	51	6	19.5	1.95	62	51
" +0.5	13.5	1.35	53	29	20.6	2.06	64	7
Level 0.0	14.6	1.46	55	36	21.5	2.16	65	3
Down +0.5	15.6	1.56	57	21	22.8	2.28	66	19
" +1.0	16.7	1.67	59	6	23.8	2.38	67	13
" +1.5	17.8	1.78	60	41	24.9	2.49	68	7
" +2.0	18.9	1.89	62	7	25.9	2.59	68	53

The curves themselves were drawn on a relatively large scale, in order to enable them to be drawn more accurately and to ensure greater precision in all computations made by reference to them. In the original diagrams the speed values (m.p.h.) are drawn to a scale of 10 millimetres per mile and the time values (seconds) to a scale of 5 millimetres per second. The total chart, showing the run diagrams for all the express runs. is nearly 10 metres long and is 0.7 of a metre wide.

The curves were prepared with a view to being made as nearly expert-proof as possible. These curves constituted a part of the technical work recently submitted before the Railroad Commissioners, and the expert testimony corroborating them, as well as the manner in which they withstood the desperate attempts to discredit or criticize them on the part of the New York, New Haven & Hartford Railroad Company before the Railroad Com-

missioners, form, I think, a sufficient credential to entitle them to your consideration.

These diagrams and the tables will serve to make my meaning clear in regard to the various assumptions made in Dr. Hutchinson's paper, to which I have previously called attention.

Let us briefly consider the various points. As to the first point, it is well-known that no line is absolutely straight, and it is also well-known that the curves increase the resistance to traction in a manner which varies with the sharpness, or the " degree," of the curve. The increased traction resistance due to curves tends to change the slope of the speed-time curve, and, consequently, affects the form of this curve the same as an up-grade; its influence does not end here, however. The increased traction resistance is, indeed, but a secondary consideration, introduced by the railroad curve. The principal consideration is the limitation which it sets to the speed that may be safely maintained on those curves. This limitation does not operate so as to introduce any material modifications in ordinary tramcar practice, but it becomes a very serious and important consideration in rapid transit service. Unless the curve occurs at the beginning or at the end of the run, on which portions the speed is relatively low, it will be necessary to arrange matters in such a way that the speed will not exceed a certain limit at the time when the car reaches the point at which it enters the curve. This could be easily done by having a motor whose limiting speed would not exceed the safe speed for the curves; but it is unnecessary to state that this would be inadmissible, because the motor would be entirely too slow to make time on the straighter portions of the run. It is necessary, therefore, in such cases, for the car to accelerate to a relatively high speed, and for this speed to be then reduced either by coasting or braking, or both, so as to bring it down to the proper limit at the time of entering the curve. It may be necessary again to turn on the current and again accelerate, after the curve is passed, in order to maintain the schedule speed. The presence of curves, therefore, introduces what are called " notches " or " humps " in the speed-time or run-curve. These notches not only affect the form of the curve, but also have an important bearing on the energy consumption in watt-hours per ton mile. The paper of Dr. Hutchinson ignores them entirely.

One such " hump " is shown in Fig. 2, in the curve A, where the speed is reduced purposely before entering a curve of 3 degs. radius, after passing over which, the electric current is again turned on so as to accelerate the speed again for a short time before coming to the next curve, also of 3 degs., in anticipation of which the acceleration is curtailed, and some coasting is again allowed.

As for the second point, it is again well-known that there is not a railway that is absolutely level from end to end, and it is also well-known that grades materially affect the power required at different parts of the line. The principal point of interest in

connection with the influence of grades in this case is the fact that a car accelerating on a grade will have its rate of acceleration modified by the grade—that is to say, the slope of the first part of the speed-time curve, or what my associate, Mr. Mailloux, designated in the Buffalo discussion as the "initial angle of acceleration," with a given equipment having a definite gearing ratio, depends upon the grade. Now, we all know that with the present resistance method of speed control the energy consumed in a given time at starting is constant, independently of the speed, up to the point at which the regulating or starting resistance is all cut out, at which point the acceleration begins to follow the so-called "motor curve." Yet the rate of acceleration, that is to say, the initial angle of acceleration, and the distance covered up to the point at which the resistance is all cut out, may differ considerably, according to the grade. This means that the speed attained within a given time and with a given energy input will depend on the grade. I have tabulated in Table I the values of the acceleration coefficient (see column headed "Coeff."), which is numerically equal to the tangent of the angle of acceleration for the eleven express runs of the proposed New York & Port Chester Railroad going in the direction of Port Chester. The highest value, 2.36 per cent., corresponding to an angle of 67° 2′, will be found in start No. 7, which, as shown in per cent. grade column, is on a down-grade of 0.9 per cent. The lowest value, corresponding to an angle of 61° 1.8′, is obtained at start No. 6, which is on an up-grade of 1.714 per cent. In starts Nos. 1 and 2, which are on a level line, the value is 2.15 (angle equals 65° 3′). Table II. gives the values of the acceleration angles and coefficients for two different kinds of motor equipments on up-grades and down-grades of 0.5 per cent., 1.0 per cent., 1.5 per cent. and 2 per cent., respectively.

It will be seen from this table that a 2 per cent. grade, for instance, changes the acceleration coefficient materially. With the 300-amp. motor the accelerating rate (coeff.) of 1.46 on the level falls down to 1.02 on an up grade of 2 per cent. and rises to 1.89 on a down grade of 2 per cent. With the 400-amp. motor, the accelerating coefficient which is 2.16 on the level is 1.74 on an up-grade and 2.59 on a down-grade of 2 per cent.

As to the third point, it may be said that while there is no material error in assuming constant train resistance for all cases where the speed does not exceed 25 m.p.h., yet above those speeds there is a possibility of error, which becomes a certainty when the speeds reach 40, 50 or 60 miles, the amount of error increasing with the speed. This matter is now so well understood that a mere mention is sufficient. Very little reasoning will show that the increase of train resistance as a function of the speed cannot be left out of consideration in any computations relating to high-speed service. It was shown by Mr. Armstrong, in his very able paper before this INSTITUTE in 1898, that for a given run the maximum speed reached during the run will increase as the angle

of initial acceleration decreases. Now, as the train resistance is a function of the speed which is higher than the first power (usually comprised between the values of exponent = 1½ and exponent = 2) it would follow that the energy actually consumed in the movement of the train is greater with low accelerations according to resistance values, because the maximum speeds attained and maintained during the run are higher. This is shown graphically by special curves in Fig. 2, which, for the sake of clearness, are reproduced separately in Figs. 2a and 2b. In these diagrams, Figs. 1, 2, 2a, 2b and 2c, the letter A designates, in every case, curves having reference to a 52-ton motor car equipped with four motors and using 400 amps. in each motor in starting, while the letter B designates in every case curves having reference to a 50-ton motor car equipped with four motors each using 300 amps. at starting. The curves A and B are the usual speed-time or run curves corresponding to these two kinds of equipment.

The curves I.-A and I.-B are the distance curves corresponding to these run curves—the ordinate value at any point on the distance curve, as read on the scale of miles at the right hand of the diagram, being equal to the distance traversed by the corresponding train at the corresponding time point.

The curves II.-A and II.-B are the power input curves, showing the electric power applied to each car at every instant of time during which current is used. It will be noticed that while in Fig. 1 the current is applied in each case only once, namely, at the beginning of the run, being shut off when the maximum speed has been reached, in Fig. 2 the current is again turned on for the run marked A, corresponding to the 400-amp. motor equipment. The area of these power curves is, of course, equal to the energy consumption, usually expressed in kilowatt hours.

Curves III.-A and III.-B are the energy,or kilowatt hour curves corresponding to the preceding curves and obtained by the integration of the same, the values being read by reference to the special scale (kilowatt hours) at the right-hand side of the diagram.

Curves IV.-A and IV.-B are special curves, to which we have given the name of intrinsic power curves. These curves show the power, in this case being expressed in kilowatts. These curves are calculated by reference to a simple and well-known formula expressing the relation between the speed and the power required to keep the car moving at that speed. The curves show two notches, which are due to different causes. The first, or smaller notch, is due to the increase in train resistance caused by the addition of the motor and gearing friction, which are both excluded, being counted as part of the motor efficiency while the car is running. This notch occurs at the time-point when the current is shut off. The second notch corresponds to the point at which the motor leaves the down-grade on which the start was made. The moment that the car leaves the down-grade,

the energy required for traction, of course, instantly increases. In the case of an up-grade, the notch would have shown a downward slope instead of an upward slope at this point, the height of the notch being, of course, in proportion with the percentage of grade.

These curves are calculated by reference to formulæ, which include the variation of the train resistance as a function of the speed. This explains their convexity toward the time-axis at the beginning and at the end.

Curves V.-A and V.-B are the integral curves of the preceding curves, which, consequently, give at each time-point the energy consumed in train resistance up to that point. The ordinate values for the power curves are expressed in kilowatts, which may be read by the kilowatt scale at the left-hand side; the ordinate values of the energy curves V.-A and V.-B are expressed in kilowatt-hours and may be read by the energy scale at the right-hand side of the diagram.

Curves VI.-A H and VI.-B H are the corresponding intrinsic power curves, drawn according to the assumptions made in the paper of Dr. Hutchinson. These curves are calculated on the assumption that the line is level and that the train resistance is constant at 18.2 lbs. per ton, as stated in Dr. Hutchinson's paper.

Curves VII.-A H and VII.-B H are the corresponding intrinsic energy curves. These curves are reproduced separately, together with the corresponding run curves, which are exactly the same as in Fig. 2 (in Fig. 2b for the sake of greater clearness). The intrinsic power and energy curves, calculated by our own formula, are also separately reproduced in Fig. 2a. A simple glance suffices to show the radical difference between the curves shown in Fig. 2a and those shown in Fig. 2b. With the assumption of constant train resistance, the beginning and the end of the intrinsic power curves of Fig. 2b are very much higher than they should be, while they are very much lower at the middle points corresponding to the highest speeds atttained during the run.

It will be observed that while the dotted line curve (V.-A) in Fig. 2a, corresponding to the intrinsic power of the train having higher acceleration, shows higher ordinate values than the solid line curve (V.-B) in the early parts of the run, yet it shows lower values at the end of the run, the two lines crossing at a time-point about midway in the run, as clearly shown on the diagram. The total intrinsic energy is slightly under 3 k.w. hours for the higher acceleration and somewhat over 3 k.w. hours for the lower acceleration. On the other hand, the corresponding energy curves in Fig. 2b retain throughout the entire run such relation that the dotted line curve (VII.-A H) always remains above the solid line (curve VII.-B H) throughout the entire run. From the forms of the energy curves in Fig. 2a, which show clearly that the energy actually consumed in maintaining the speed is less for a high acceleration than for a low acceleration, one would be led

to look for greater economy with higher acceleration; whereas, from Fig. 2b, one would be led to expect the contrary result. These curves illustrate the manner in which an unwarranted assumption may lead to erroneous conclusions.

As to the fourth point, namely, the average length of run, in the case of the New York & Port Chester Railroad, the average length of run is 1.9 miles (10,032 ft.) for the express train runs, and 0.95 mile (5,020 ft.) for the local train runs. The longest express train run is 3.114 miles and the shortest 0.689 mile. The first column in the table gives the length of all the eleven express runs in miles. It will be seen that the longest run (No. 4) is 3.114 miles and the shortest one (No. 5) is 0.689 mile. In the case of the local runs, the longest one is 1.853 miles and the shortest is 0.428 mile. It will be seen that there are not two of the express runs which are of the same length and that the range of variation is great. The same statement applies to the local runs. Referring again to the values of the factor which Dr. Hutchinson designates by the letter "*A*" and to which he gives the term "through acceleration," it will be seen that this value also varies widely, its highest value (for run No. 3) being .614 and its lowest value (for run No. 4) being .234. The variations of this quantity "*A*" are such as to show clearly that it is not an independent variable, but that it is itself a function of other things, such as the length of the run, the maximum speed attained, or of some other quantity.

In regard to the fifth point, namely, the assumption that the power will be applied during the first part of the run only, it may be said that this is somewhat related to the first, that is to say, it depends on the relative straightness of the line and has already been referred to in that connection. The necessity of reducing the speed either by coasting or braking so as to enter a sharp curve at or below a certain definite limited speed, involves in most cases the necessity of again putting on the current and of again accelerating or raising the speed before coasting. An example of this is shown in Fig. 2, in the run in curve A already referred to, where the car, after attaining a speed of 53 m.p.h., is allowed to coast, so that by the time the car enters the curve the speed has fallen to 42½ miles. The limiting speed for this curve (3° of curvature) being 50 m.p.h., the car continues to coast until the speed has fallen to 41.1 m.p.h., at which time the current is again turned on and the speed is accelerated to 45 m.p.h. The diagram shows that the energy input curve, corresponding to this second acceleration, requires an expenditure of 0.62 k.w-hours, which, added to the energy consumed in the first acceleration (6.02 k.w. hours), makes a total of 6.64 k.w. hours as the energy required for the run. In some runs it may be necessary to have three, four, or more successive accelerations. These supplemental accelerations are left out of consideration entirely in Dr. Hutchinson's paper, although they are evidently of too much importance to be thus slighted in any comprehensive

solution such as this is asserted to be. The difficulty of dealing with them is increased by the fact that the percentage of energy due to these successive accelerations depends entirely upon the peculiar conditions of the case, and cannot in the present state of the art be foreseen or predetermined in any other way than by actually constructing the speed-time curve under the exact conditions. It is of interest to note in this connection that in Fig. 2, in the run corresponding to the motor having higher acceleration, the total energy consumption for the entire run is still lower than that required for the entire run with the equipment of the lower acceleration, notwithstanding the fact that, in the former case, a consequent acceleration occurs which involves an additional energy output of 0.62 k.w. hours. I have also brought with me, and will add to the discussion, four additional run diagrams (Figs. 3, 4, 5 and 6), two of which (Figs. 3 and 4) show notches due to the necessity of reducing the speed on entering curves and requiring subsequent acceleration, thus involving additional energy input. These curves give interesting information as to the expenditure of energy required for these successive accelerations. These curves and the other curves also show incidentally some interesting results of the influence which a small increase in the time of the run exerts on the energy required for this run. The table on each diagram gives the principal data and results in regard to each diagram.

As to the sixth point, namely, the use of a speed-time curve of general type for all cases and conditions, it is my opinion that such a curve, simplified as it must necessarily be, involves too many assumptions to be a safe and desirable expedient. Without analyzing very definitely the character of the correction to the type curve which the author suggests and proposes to apply, I will say that I am inclined to look upon this correction with suspicion, not only because it ignores the important points just discussed, but because of the possibility of error in the computation of the resulting energy input curve. The correction, as I understand it, consists in reducing the actual curve to an equivalent typical curve, in which the entire acceleration is along a straight line, as shown by the dotted line M A A. It seems to me that this correction leaves room for doubt as to the form of the energy input curve, and, consequently, leaves room for more or less important error in the estimated energy consumption. I surmise that this is, in fact, another one of the weak points of Dr. Hutchinson's hypothesis.

The seventh point requires no special discussion, as the discussion of the preceding points has already made its fallacy sufficiently evident.

I now come to the feature of Dr. Hutchinson's paper, which is perhaps of the greatest practical interest, namely, the somewhat radical conclusion which he arrives at, to the effect that, contrary to what has been hitherto believed, the economy of high acceleration is of secondary importance, if not negligible.

For instance, the author says: "I have made these calculations for a number of cases to correspond with practical conditions; the result is always that the energy used per mile is less for the lower accelerations." On this point I absolutely disagree with Dr. Hutchinson. The comparison already referred to between the curves in Fig. 2a and Fig. 2b is, as we have seen, opposed to this conclusion. The comparison of the energy curves in Figs. 1 and 2, in my opinion, contradicts Dr. Hutchinson's conclusion entirely. The two run-curves in each diagram, it will be noticed, are curves of equal area as well as of equal time, that is to say, the acceleration and coasting are arranged in both cases in such a manner that the run is made in the same time.

The curves show readily the necessity for higher maximum speed when using the equipment consisting of smaller (300-amp.) motors, giving a lower acceleration; and the energy input curves (3a and 3b) show a material difference in the energy consumption per ton-mile during the run, which difference is all the more striking if we take into consideration the fact that the curves (B) corresponding to the smaller motor involve a total train weight and an intrinsic energy expenditure corresponding to only 50 tons total load, while the curves (A), corresponding to the equipment of larger (400-amp.) motor, involve a total train weight of 52 tons. As would be expected, and as shown by the comparison between Figs. 1 and 2, the shorter the run (Fig. 1) the greater is the economy of high acceleration.

The energy required for the high acceleration run in Fig. 1, as shown by the area of the large energy diagram furthest to the left, is 4.49. The energy required for making the same run under the conditions referred to in curve B, is that shown by the energy diagram furthest to the right, and is equal to 5.32 k.w. hours. The data for both sets of conditions are given in the upper right-hand corner of the diagram.

The figures for the energy input per car mile corresponding to curves A and B are, respectively, 6.52 and 7.72 k.w.hours—the corresponding figures for the energy in watt-hours per ton-mile being, respectively, 125.4 and 154.4. The increase of energy required for the lower acceleration is, therefore, 23.1 per cent. greater with the smaller equipment giving the lower acceleration. The same comparison in the case of Fig. 2 shows that the increase of energy in watt-hours per ton-mile in the case of curve B, as compared with curve A, is 18.4 per cent.

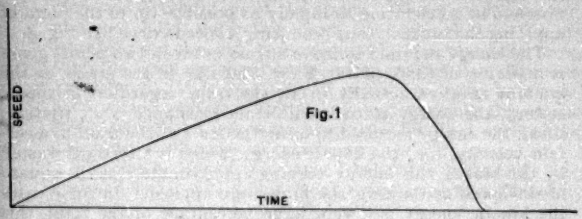
In discussing the New York Subway Rapid Transit road, the author concludes that the proposed schedule is impracticable, and he states that a motor capacity of 14.9 k.w. per ton is beyond the range of practical conditions. As the conditions in the case of the New York & Port Chester Railroad express service are, if anything, more severe, owing to the higher schedule speed, it would seem that the practicability of this project would also fail to meet the requirements of Dr. Hutchinson's formulas. The 52-ton car has an equipment of four 400-amp. motors, operating

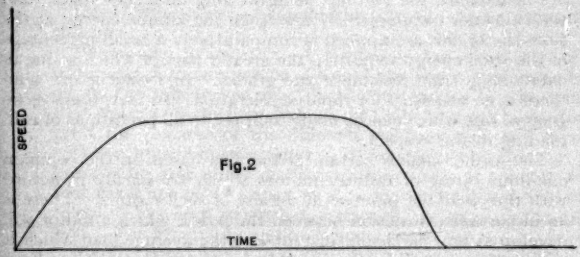
at 600 volts, making an input of 960 k.w., or about 18.5 k.w. per ton. If there should be a discrepancy between what we have found to be possible and what the hypothesis presented in this paper admits or limits, I, for one, think that the adjustment necessary to bring the two into harmony will have to come largely, in fact one might say wholly, from the side of theory.

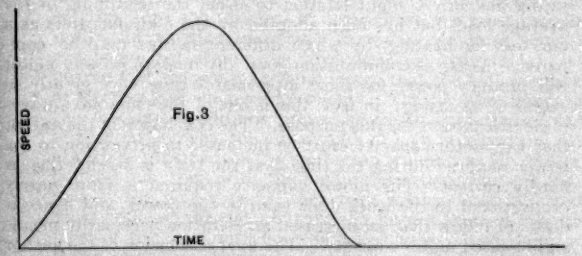
MR. M. H. GERRY, JR., [*Communicated*]:—The paper by Dr. Hutchinson contains a very interesting and able theoretical discussion of certain problems in connection with moving trains by electricity. While any general discussion on this subject is of great value, it does not seem that this paper embraces a sufficient number of the operating conditions to justify such general conclusions as stated by the author. In fact, it is highly improbable that any complete mathematical discussion can be had which will give correct general conclusions on these points. The conditions of practical railroading are so varied, and the results to be obtained depend to such an extent upon the traffic arrangements, that it is almost essential that each particular case be considered by itself. On certain elevated and suburban roads, the question of schedule speed is of first importance, as on it depends the amount of traffic which can be handled, and hence the amount of income received. In a case of this kind it is not a question of power, but rather one of maximum schedule speed which can be regularly and safely maintained, with stations at certain fixed distances apart; and the more rapid acceleration obtainable is usually the most desirable, for the reason that the fastest schedule can be obtained by the highest rate of accelerating, up to the point of applying the brakes, and then retarding as rapidly as possible, until the train is at rest.

As far as train efficiency is concerned, it is of course,, well-known that the least energy will be required with the highest acceleration up to a point at which the power is cut off and the train allowed to coast until such a speed is reached that the brakes, by causing the maximum retardation, will just bring the train to rest at the proper point. Such theoretical conditions are, however, rarely either the most economical or the most desirable ones in practice. As a usual thing, certain distances between stations, maximum speed, number of trains, amount of traffic, etc., are determined by conditions entirely outside of those of power, and it then becomes the problem to operate a train service as economically as posisble. The curve (Fig. 1), from a power standpoint, represents conditions about as poor as it is possible to obtain, but nevertheless it was the best obtainable with certain steam locomotives on a suburban division. If it had been possible to accelerate more rapidly and then either coast or maintain a nearly constant speed until the point of applying the brakes was reached, a great saving might have been effected; but owing to the motive power and traffic conditions, the particular curve shown was the best that could be obtained. On many surface roads, and especially on street railways, the

maximum speed is the limiting factor. In a case of this kind it would be the best practice to make use of a moderately rapid acceleration, up to approximately the maximum speed allowed, and then to maintain this speed until it became necessary to







apply the brakes in order to make the required stop. This would produce a flat-topped curve something like Fig. 2, and while not representing the most economical conditions from a power standpoint, it would still represent the most desirable practical

conditions, as it would produce the highest schedule speed, keeping within the limits of maximum speed, and with the smallest consumption of energy, under these conditions. Where maximum schedule speed is of the first importance, and there are no limitations as to maximum speed, then of course, it is merely a problem of accelerating as rapidly as possible, up to the point of applying the brakes; thus producing a speed-curve like Fig. 3.

The energy required to move a train between two points given is made up of three parts First, that due to the grade, or the amount required actually to lift the train, regardless of speed; second, the energy to overcome train resistance, *i. e.*, friction; third, the energy required to bring the mass of the train to a certain velocity, *i. e.*, the kinetic energy, which is afterward wasted by the brakes, this energy varying approximately as the square of the speed at the time the brakes are applied. In some cases the kinetic energy is a very large percentage of the total, this occurring where the distance between stations is short and the velocity relatively high. On the other hand, where the stops are infrequent, the stations being at long distances apart, and the velocities comparatively low, then the kinetic energy at the time the brakes are applied is comparatively a small percentage of the total energy required; the greater part of which is due to overcoming train resistance and grades. In a case of this kind there is no necessity for rapid acceleration. In fact, there is no general rule which can be made to apply to all conditions of railroading in this respect.

The author makes certain deductions based on the common one-hour rating of railway motors which are hardly in accord with the results of practice in the use of such motors. There is an important distinction between the power which a motor will develop at any particular instant and the average load which it will carry on an all-day basis. As a matter of fact, the one-hour rating has only a slight relation to either the maximum or the average load, but has been adopted by the manufacturers as a fair test for heating, by which different motors may be compared. As far as commutation goes, any modern railway motor will produce power for short intervals of time very greatly in excess of its rating; in fact, this is one of the chief advantages of electric motors for this purpose. The conclusion of the author that the motor capacity must be increased in proportion to the power required during the time that the train is accelerating, is hardly correct. The motor capacity required is more nearly proportional to the work done than to the power, and hence it does not follow that an increased acceleration necessarily means an increased motor capacity. The final conclusion of the author of this paper that the lowest acceleration is best under practical conditions is not in accordance with either the experience or the deduction of most engineers who have had under consideration problems of this kind. Furthermore, the conclusion that the single advantage for rapid acceleration is the saving in energy

is hardly borne out by practice, where often the more rapid schedule speed obtained is of even greater importance than the saving of energy. Then, too, it cannot be admitted that the investment is necessarily greater, that there is an increased cost of maintenance, or that the station load factor is poorer, with increased acceleration of the trains. These conditions may, or may not hold, depending more upon the design, the system adopted, and the ends to be obtained, than upon the mere fact that the train acceleration is either increased or decreased in any degree. It is desirable, of course, to avoid extremes and keep within practical limitations, and in doing this it may be safely said that there is no fixed rule, but that each particular case must receive special treatment.

The conclusions of this paper I believe to be entirely too general and to be misleading, as applying to certain classes of railroading.

PRESIDENT STEINMETZ:—The previously announced discussion is here finished and discussion is open to all who desire to make any further remarks.

DR. HUTCHINSON:—I should like to make some statements at this point.

PRESIDENT STEINMETZ:—Considering the character of the paper and of the discussion, I would like you to understand that we do not relieve you from further discussion by the remarks that you may make here, but expect that when you have seen in print the remarks of the different speakers, you will favor us with a complete answer, after fully considering all that has been brought up here in the discussion, the more as it appears to me that the scope of the paper, as a general and not a special investigation, has been somewhat overlooked in some of the discussion. The general discussion is now opened.

MR. PHILIPPO TORCHIO:—Dr. Hutchinson has presented a very interesting and able paper, clearly stating the different factors entering into the problem of determining the most economical results for traction in cities. However, the most economical results are not always the most desirable in practice. It is a proved fact, known for several years, that the high acceleration at starting gives increased economy. On the other hand, under the practical conditions prevailing in the operation of surface lines in large cities, the advantages of high acceleration cannot always be obtained, as the motorman will not, as Mr. Potter has pointed out, start with the high acceleration, shut off the current at the proper point, then take a long coast and make a short brake at the end; but usually he will keep on the current most of the time until he is signalled to stop and then he will brake abruptly. Under practical tests made by railroad engineers on several lines, it was proved that the 23 per cent. increase of economy at a high acceleration, given by the speaker of the evening, under practical running conditions, was reversed to 6 per cent. increased energy consumption. The comparative test

was made, using motors of about 50 per cent. larger capacity than were used before, the running schedule and other things being maintained equal. However, practical considerations other than that of the saving of energy, led these engineers to adopt the larger capacity motors in place of the motors most economical as to energy consumption, station capacity and copper investment.

MR. H. G. STOTT:—I think, as a first proposition, it is thoroughly recognized by all railroad men that maximum acceleration gives maximum economy. A number of points have been brought out, but that one, it seems to me, is the principal one that everybody has to recognize. Another point brought up has been the question of stored energy in the motors. Some time ago I saw some investigations carried out on that subject, and while theoretically it developed that there was a distinct loss of energy due to fly-wheel effect, or stored energy in the motor, yet in an actual test, under working conditions, these results were not corroborated and the practical test with ten-second readings of instruments, did not show that the results of the calculations were corroborated. There are a great number of points to be considered. For instance, looking at the speed-time curves, the stored energy may be recovered by cutting off a little sooner and coasting, and in that way it might be a positive benefit to have a larger armature and therefore store more energy; and that subject, I think, would be a very good topic for a complete discussion for an entire evening, because there are so many different phases of the question. But that is one practical result that we get in investigating it, that the theoretical calculations were not borne out. In looking over Dr. Hutchinson's paper I must agree with the rest of the critics in saying that I do not quite see where it applies to existing railway conditions.

PRESIDENT STEINMETZ:—Dr. Hutchinson's paper considers the operation on a level, straight track, with equidistant stops. Those are not the usual railway conditions. I agree as to that. But so is the standard test of an alternator on non-inductive load, not representing operative conditions, but the operative conditions will infinitely vary. You may have all kinds of load, but you must have some definite standard from which, by knowing the particular conditions of your case, you can by judgment derive the results. This, for the alternator, is non-inductive load, although very few alternators in commercial service will enjoy a non-inductive load. So here, from the performance of the railway motor on a straight, level track, with equidistant stops, we may by knowing the particular severity of our service as regards curves and grades derive by judgment what the motor will do in actual service. We may be mistaken in our judgment, but surely we are very much nearer than if we knew nothing at all. If we know exactly the conditions, the profile of the road, the curves and grades, the character of the motor-

man who is going to operate the car, etc., then we can get very much more accurate results, undoubtedly, but where we do not know the actual conditions, we are permitted by judgment to reach a conclusion regarding the behavior of the motor more accurate than by the former guessing. That, I understand, is the purpose of the paper.

Before we adjourn I may say that it is proposed to have at the Annual Convention one meeting devoted to the electric railway, as it is a very important matter, so whatever some of you may remember afterwards as very interesting for discussion here, we can discuss at that time.

MR. S. T. DODD:—I wish to express my appreciation of the method and results of this paper. Many of us have spent a good deal of time juggling with motor curves and working out results for particular cases. General formulas of universal applicability for determining the distance transversed, the energy expended, the speed, etc., for problems in motor acceleration have been needed for a long time and if, as appears from this paper, the writer has developed such formulas of convenient and general application, he has made a decided advance in the study of such problems. I do not feel sufficiently familiar with the paper to criticize its general results or to say how convenient the formulas may be for every day use, but it appears that the results should be of great assistance to us.

Some time ago, I spent a good deal of time on the investigation of some problems in acceleration, and in view of the results arrived at in that investigation, I have been particularly interested in the conclusion of this paper, namely, that the lowest acceleration possible under "practical conditions" is the best. It appears to me that there is a possibility here of a very interesting general investigation to carry this conclusion further and to determine what are the practical conditions under which one or another acceleration is advisable.

This thought has led me to take up again my old problem and apply to it the formulas of Dr. Hutchinson's paper and if my results differ from his, it is to be attributed to the assumptions that I have made in my particular case and to the fact that I have developed my investigations along slightly different lines from his, or perhaps to the fact that I have not appreciated the limitations of his formulas and curves.

Let us consider a car equipped with motors of such a capacity as to give it an acceleration of 2 m.p.h. per second, up to 15 m.p.h. at rated load of the motors. This is an acceleration which is ordinary, or at least, is frequently met with in practical conditions.

We will assume that during acceleration, we do not exceed the rated load of the motors, *i. e.*, while cutting out resistance, we work along the line $a = 2$ at the point 100—100 of curve sheet 5 and that at the point where all resistance is cut out, the speed, $\beta\ 100 = 15$, therefore $\beta = .15$. Assume, moreover, that b, the

TABLE A.

$a = 2$ $\beta = 15.$ $L = 1500'$

Time Seconds	Speed m.p.h.	Distance Feet	Energy w.h.	Schedule Speed m.p.h.	W.h. per ton mile.
7.5	15.	82.5	11.75		
12.	19.5	195.	17.4	10.05	61.7
16.5	21.6	330.	21.3	12.36	75.
21.	22.8	472.	24.4	13.16	86.
25.5	23.8	622.	27.1	13.65	95.5
30.	24.7	792.	30.	14.02	105.
34.5	25.35	982.5	32.45	14.28	114.2

TABLE B.

$a = 1$ $\beta = .227$ $L = 1500'$

Time Second	Speed m.p.h.	Distance Feet	Energy w.h.	Schedule Speed m..h.	W.h. per ton mile.
27.7	27.7	565	43.6	13.05	153.5
31.85	31.	756	50.9	14.	179.
36.	33.	947	55.9	14.15	196.7

TABLE C.

$a = 3$ $\beta = ..103$ $L = 1500'$

Time Seconds	Speed m.p.h.	Distance Feet	Energy w.h.	Schedule Speed.	W.h. per ton mile.
3.44	10.3	26	5.38		
15.80	17.92	281	14.6	8.48	50.7
17.86	18.5	334	15.66	10.27	55.1
19.92	19.	392	16.71	11.1	58.8

negative acceleration during coasting, equals .2, or the train resistance is equal to 18.2 lbs. per ton. Apply now formula (18) the power developed per ton by this equipment is $P = .182\ (a+b)\ ax = 6$ k.w. or 8 h.p. This would correspond for example, to a 15-ton car equipped with two 60 h.p. motors. We will assume that this equipment is making a run with a distance of 1,500 feet between stations. The energy expended, distance

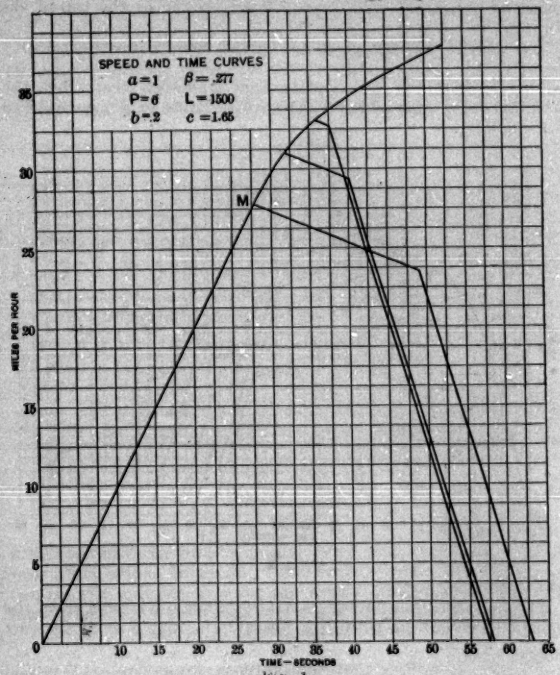


Fig. 1.

traversed and speed of the equipment may be determined directly from curve sheet 7. Now, the question I wish to investigate is: Suppose the same equipment was geared for a higher or lower speed, but was started in every case at its rated load, what would be the effect of this change of gearing on energy consumption, schedule speed, etc.

($a = 2$). We will study first the case where the car is geared for an acceleration $a = 2$ at its rated load. The results can be

derived from curve sheet 7 and are tabulated in Table A. The first row of figures in this table corresponds to the time up to the point M on the speed curves where all the resistance is cut out. The succeeding rows correspond to the values 30, 60, 90, etc., of x on curve sheet 7.

The first column gives the time elapsed since the starting, and is derived by multiplying each value of x by β and adding the result to the initial value of t (7.5 sec.) given at the top of the column.

The second column gives the speed, read from the velocity curve.

The third column gives the distance traversed, read from the distance curve and added to the initial value of distance 3,670 β or 82½ feet.

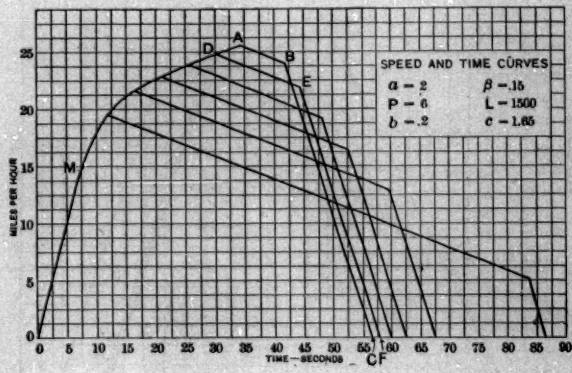


Fig. 2.

The fourth column gives the energy expended, read from the energy curve and added to the initial value 522 β or 11.75 watt hours.

Now let us assume that the power is shut off at any one of these points and the car is allowed to " coast " and finally braked and brought to rest at a distance of 1,500 feet from the starting point. We will assume the frictional retardation (b) is .2, corresponding to 18.2 pounds per ton and the braking retardation (c) is 1.65, corresponding to 150 pounds per ton. Knowing from Table A the velocity V and the distance to be traversed during coasting and braking $(1{,}500 - d)$, we can find y, the time during which the car will coast and z, the time during which it will be under braking retardation.

The formulas connecting these two quantities are as follows:

$$b\,y^2+c\,z^2+2\,b\,y\,z = 2\,(1500-d)\,.682$$

$$b\,y+c\,z = V$$

$$\therefore z^2 = \frac{V^2-1.364\,(1500-d)\,b}{c\,(c-b)}$$

$$y = \frac{V-c\,z}{b}$$

The values of V and d from Table A, substituted in the first of these equations gives the time z during which braking will take place. Values of V and d such that V^2 is less than $1.364\,(1,500-d)\,b$, give imaginary values of z; *i. e.*, indicate that the car

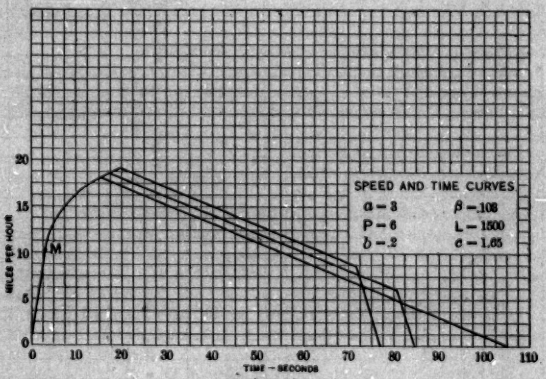


Fig. 3.

if allowed to coast from such a velocity, will stop from frictional resistance before traversing 1,500 feet. Values for which V is less than cz indicate that the car, even if braked with "no coasting" would have traversed over 1,500 feet before being stopped.

Let us first assume the power shut off at the time $t = 34\frac{1}{2}$ sec. Applying the formulas for y and z we find the car will coast 7.5 seconds (y) and be brought to rest under the brakes in an additional 14.45 seconds (z). The relative amounts of acceleration, coasting and braking are most satisfactorily shown in the "speed-time curves" for $a = 2$, by the curve O, M, A, B, C. The intercept on the base line shows that the total time the car has been in motion is 56.45 sec. Adding to this 15 sec. for the stop at the station, we get the schedule time for a run of 1,500 feet, from which the schedule speed 14.28 m.p.h. may be calculated. If the power is shut off at the time $t = 30$, the coasting curve is longer

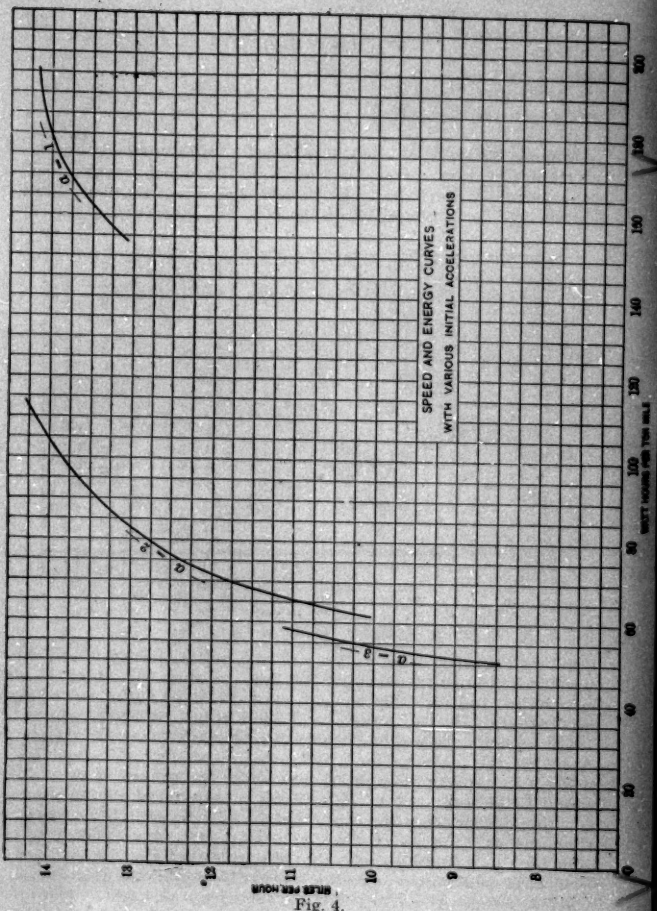

SPEED AND ENERGY CURVES
WITH VARIOUS INITIAL ACCELERATIONS
a = 1
a = 2
a = 3
D
WATT HOURS PER TON MILE
MILES PER HOUR
0
20
40
60
80
100
120
140
160
180
200
8
9
10
11
12
13
14

Fig. 4.

and consequently the schedule speed is less. This is shown by the curve O, M, D, E, F, of the same sheet.

The successive straight lines starting from the acceleration curve on this sheet show the effect of shutting off power and beginning coasting at the successive times shown in Table A and the schedule speed, calculated from the intercepts on the base line are tabulated in the fifth column of that table.

Values of V and d at the time $t = 7.5$ give imaginary values of z showing that the car would not cover 1,500 feet if allowed to drift from this point.

As said above, the fourth column of Table A gives the energy expended per ton up to the time under consideration. If the power is shut off at this point, this represents the total energy used in traversing 1,500 feet; multiplying this by 3.52 gives the energy expended per ton for traversing one mile. The sixth column of Table I gives the watt hours per ton mile thus calculated.

($a = 1$). Let us now assume this equipment geared for a higher speed and lower horizontal effort, so that at its rated load it will develop an acceleration $a = 1$. Since the horse power of the equipment is the same as before, we have from formula (18)

$$P = .182\,(a+b)\,ax$$
$$6 = .182\,(1.2)\,\beta\,100$$
$$\beta = .277$$

The results of this are tabulated in Table B and illustrated in the "speed-time curves" for $a = 1$. The values in the Table B are derived from curve-sheet 8, as those in Table A were from curve-sheet 7. It will be seen that only a few readings are possible, as beyond $t = 33$ sec. we get negative values of y. The "speed-time curves" show that this means that nearly all the time is spent in acceleration up to the motor curve and in braking down from it.

($a = 3$). Let us now assume the equipment geared for a low speed and high horizontal effort, so that at its rated load it will give an acceleration $a = 3$. Again from equation (18) we derive the value of $\beta = 1.03$, and from curve-sheet 6 we derive the values of distance, energy, etc., shown in Table C. Here again, we are limited to only a few readings, as those below $t = 15.8$ seconds give imaginary values of z, showing that the car would not run 1,500 feet if power was shut off below this point. I have not attempted to carry the curves much beyond the points shown on curve-sheet 6. It would be interesting to study the results in this case beyond this point, but the readings we have, are sufficient to show the character and relations of the curves.

We are now in a position to answer the question we started to investigate, as to the relative amounts of energy used by operating with different accelerations. We have the data in the fifth and sixth column of each table from which to plot the relation between schedule speed and energy expended for the different

rates of acceleration. This has been done on the sheet of "Energy and Speed Curves." The curves on this sheet show for any given initial acceleration the effect on the energy consumption of introducing more or less coasting in operation and thereby modifying the schedule speed. Two or three points are worth noting in these curves.

(*a*). The watt hours per ton mile vary from about 50 to 200 according to the character of the acceleration and amount of the drifting.

(*b*). The energy is a minimum at the "no braking" point which is the minimum speed and increases with the speed, slowly at first, but quite sharply as we approach the "no coasting" point. This is to be expected, as the higher values represent greater losses in brake friction.

(*c*). The $a = 2$ curve is shown throughout nearly its whole length from the "no braking" point to the "no coasting" point.

The $a = 1$ curve is shown from the beginning of the motor curve to the "no coasting" point.

The $a = 3$ curve is shown only a short distance up from the "no braking" point. It would be interesting to carry the curve further and see its character at higher schedule speed.

(*d*). Comparing the $a = 1$ curve and the $a = 2$ curve it appears that the average watts expended per ton mile with an initial acceleration $a = 1$ is nearly 100 per cent. greater than the watts expended with an initial acceleration of $a = 2$, with the same starting currents and the same schedule speeds.

The $a = 2$ curve shows an expenditure of energy about 10 per cent. greater than the $a = 3$ curve for the same schedule speeds.

Whether these curves would continue to be approximately parallel to each other, or whether as the $a = 3$ curve approached the "no coasting" point it would cross the $a = 2$ curve, can only be determined by carrying the curves of curve sheet 6 to higher values. It is evident from an inspection of the "speed time curves" for the two cases that it would not be possible to attain the same maximum schedule speeds with the $a = 3$ curve as with the $a = 2$ curve.

The conclusion which this investigation appears to indicate is that in this particular case the higher acceleration is the most economical not only in the power-house, but on equipment. You will note that we have assumed in every case that we have the same equipment and it is started with the same starting current, but in one case is geared for a high speed and in the other for a low speed, and the curves show that at the same schedule speeds, the average power consumption is less for the higher rate of acceleration.

As I understand Dr. Hutchinson's paper, his general conclusions are directly opposed to this, at which I have arrived. He is more familiar than I am with the methods and reasoning of his paper and may be able to point out the difference in our

assumptions or reasoning which has led to this apparent discrepancy. I started to follow out this investigation with the intention of testing the convenience of working from his formulas and curves and trust that the results are of sufficient interest to warrant me in contributing them to this discussion.

MR. P. O. KEILHOLTZ [*Communicated*]:—Dr. Hutchinson is to be congratulated upon his very clever analysis of the problem, but I am of the opinion that the solution is largely academic and I do not agree as to his conclusions. I consider that the most important railway engineering problem is that of increasing the capacity of the road with the view of increasing its gross revenue. This means decreased running time, and with frequent stops, the time of acceleration is a most important factor. It also means increased station, terminal and yard facilities, greater power and transmission capacity. Decreased running time requires better signalling systems, better roadbed, etc. In fact, the capacity of a road is a function of its total cost to which its power and transmission plant may bear a very small relation. It therefore follows that the problem of acceleration of trains by electric traction should be considered in relation to the total cost of the road and its cost of operation, rather than in its relation to an item of that cost and operation.

DR. LOUIS DUNCAN [*Communicated*]:—Dr. Hutchinson's paper is valuable, as it brings out clearly the fact that a maximum train acceleration does not always mean an increased economy. The factors which he has taken into account have been neglected in the ordinary treatment of the subject. A high acceleration means an increased weight, and the added weight makes the energy per car mile for the same schedule about the same for all accelerations within the limits imposed. The tests which have been published, showing increased efficiency for high accelerations, have been made on trains with constant weights and with the same motors. They are not applicable to the practical case in which the motors must be designed to fit the acceleration.

It should be distinctly understood that in the paper the schedule is fixed, and the question discussed is the best acceleration to make this schedule. The motor curves are apparently taken from actual results obtained from machines now on the market and in operation, so there should be no question of their approximate accuracy.

The paper is important because it has lifted the question of train acceleration out of a rut and put it on a level of common-sense engineering.

[COMMUNICATED AFTER ADJOURNMENT BY DR. HUTCHINSON IN REPLY TO DISCUSSION AND CORRESPONDENCE.]

This discussion shows such serious misunderstanding of the paper that I will state its scope before referring to the remarks in detail. One of the most important factors in considering train movement is the initial acceleration. My purpose is to show

the effect on the energy and motor capacity of using different initial accelerations. To this end it is necessary to keep all other conditions constant, otherwise the result is due to a change in more than one condition and cannot be estimated accurately.

I have therefore elaborated a method that gives the effect of varying the initial acceleration on the energy and motor capacity required for any fixed schedule speed over any distance. That is, I discuss the results of making, *e. g.*, a schedule speed of 15 m.p.h. for a distance of 2,000 feet, with any assumed initial acceleration. I do not discuss the differences due to various schedule speeds, although this is a simple deduction from the data given. Nearly every participant in this discussion has missed this point, and has assumed that the discussion was on the relative value of high and low schedule speeds.

To bring out the effect of varying one quantity all others must be kept constant. It is therefore necessary to use a generalized motor curve applicable to all sizes of motor. The general curve that I give is the average from a number of tramway motors now made by the two chief companies.

The criticism that these curves are not applicable by reason of the varying values of the motor losses is clearly without foundation; different motor characteristics will effect similar changes for an initial acceleration; better absolute results will be had with a better motor, worse with a poorer, but the relative results will not be altered.

The paper is based on certain assumptions; one is that the motor carries only the "rated current" during the period of initial acceleration. I have deduced the curves on various other assumptions of load during the initial acceleration up to 50 per cent., overload. The relative results are not changed materially thereby, although the motor capacity required is changed. These curve sheets were not published; they would have made the paper too voluminous.

Perhaps the best answer to the general criticism of the "theoretical" and "mathematical" nature of this paper is a comparison of the results given by these curves with results obtained independently.

In the issue of the *Electrical World* of Jan. 25, 1902, the following data are given of the equipment adopted by the engineers of the Manhattan Elevated Railway after long investigation:

Schedule speed, 14.7 m.p.h.
Length of run, 1,775 ft.
Station stop, 14 secs.
Running time, 68 secs.
Total time, 82 secs.
Initial acceleration, 1.5 m.p.h. per second.
Retardation after braking, 2 m.p.h. per second.
Energy used per ton mile, 77 watt-hours.
Train weight (equivalent) 174 tons.
Equipment adopted, 8 G. E.-66, 1000 rated h.p.

Kilowatts per ton, 4.3.
Starting current per motor, 200 amps,
Load at starting, 100 per cent. of rated capacity.

From these data, the average velocity $V = 17.7$ m.p.h., and the through acceleration $A = .26$.

The rule given in the paper is: The best initial acceleration to adopt is that acceleration giving the minimum motor capacity. Since these motors are used at their rated capacity during initial acceleration, as is assumed in the paper, the curve sheets are applicable except in so far as they are modified by the fact that the braking retardation adopted is 2 m.p.h. per sec. instead of 3, as used in the paper. This will make a very slight difference in the results.

Curve sheet 14, for $A = .26$, gives for the initial acceleration for the minimum motor capacity 1.5 m.p.h. per sec., agreeing exactly with the figures determined by the Manhattan's engineers. Curve sheet 13, at this initial acceleration, gives for the energy required 75 watt-hours per ton mile. The Manhattan's engineers find 77; the agreement is substantially perfect.

Again, from curve sheet 14, the motor capacity required is

$$.165 \times 1775\tfrac{1}{2} = 6.8 \text{ k.w. per ton.}$$

The capacity adopted by the Manhattan's engineers is 4.3 k.w. per ton. This difference is material. An examination of the curve sheets of the motor adopted—the G. E.-66—shows at once the reason. In the paper it is assumed that the sum of the core and copper losses at rated load is 12 per cent. of the rated capacity. Curves heet 14 is based on this assumption, which, as I have said before, is a fair assumption. It happens, however, that this particular motor has a very low heat loss, the sum of the copper and core losses being only 7.8 per cent. at rated load. The motor capacity depends directly upon this loss. Reducing the capacity found from curve sheet 14 in the ratio of 7.8:12—that is, the ratio of the heat losses at rated load—gives a motor capacity of 4.42 k.w. per ton for a motor such as the G. E.-66. This figure is practically identical with the value adopted for the Manhattan equipment.

In other words, the engineers of the Manhattan Railway have unconsciously followed the rule contained in this paper and have adopted, after laborious investigation, motors giving exactly the same initial acceleration and the same energy consumption as would have been determined in a few moments by the curve sheets of my paper had they been available; and when proper allowance is made for the difference in the heat losses, practically the same motor capacity per ton. The engineers of the Manhattan Company evidently do not believe in high initial accelerations.

There is little that requires a detailed reply in the discussion of the paper. Mr. Stott and Mr. Gerry both state that it is a "recognized fact" that maximum accelerations give maximum

economy. This, of course, I dispute; and as they offer no testimony to support their position, it must be taken merely as a matter of opinion. Mr. Gerry and others confuse schedule speed with initial acceleration and seem to think that I am arguing for low-schedule speeds.

Mr. Potter's paper gives interesting data on the individual characteristics of different motors, but has no particular bearing on my paper. We all know that motors differ, and for this very reason it was necessary for me to assume an average motor. Mr. Potter has probably not attempted to test the application of these curves to practical conditions, or he would not have stated his belief that they were not applicable.

Mr. Gotshall has presented a number of interesting curves showing the " runs " on his proposed New York & Port Chester road. These curves are merely paper estimates, made in the same general manner as some of my curves are made, but for specific motors. By adding a discussion of the effect of grades and curves (which I purposely omitted, since they are arbitrary), Mr. Gotshall attempts to show that the results of my paper are not applicable to practical conditions. He finds great difficulty in comprehending the meaning of the quantity " A," the " through acceleration "; in fact, I think that it would hardly be an exaggeration to say that he has not yet comprehended it. In the verbal discussion of the paper he announced his discovery that this quantity A, which he declared I had called *constant*, had very different values for the different runs of his road. After thinking the matter over he offers in the written discussion another interpretation in these words: " In his paper Dr. Hutchinson has used and applied the letter A as a symbol designating what he chooses to call ' through acceleration,' which is a purely abstract idea, the physical meaning of which is not easy to realize. . . . The variations for the individual runs in the values of this quantity are very great, the differences being as much as 200 per cent. I believe that Dr. Hutchinson intends the value of A to be a sort of average acceleration factor."

I think it is hardly necessary to comment upon these quotations.

Mr. Gotshall, again referring to my unfortunate quantity A, shows that he finds great difficulty in grasping the meaning of " independent variable." He says: " Referring again to the values of the factor which Dr. Hutchinson designates by the letter ' A,' and to which he gives the name ' through acceleration,' it will be seen that this value also varies widely, its highest value (for run No. 3) being .614 and its lowest value (for run No. 4) being .234. *The variations of this quantity A are such as to show clearly that it is not an independent variable, but that it is itself a function of other things, such as the length of run, the maximum speed attained, or of some other quantity.*" Possibly he means by " some other quantity " that it is a function of the understanding of the reader, or the want thereof.

Mr. Gotshall also seems to think that he can use such motor capacities as 18.5 k.w. per ton. Since the weight of motors, controllers, brakes and sundry electrical equipment is very approximately 70 lbs. per k.w., this means that 1,300 lbs. will be required for these parts alone. It is difficult to see how the remaining 700 lbs. is sufficient for trucks, car body and passengers. That no such motor equipments will ever be used is a safe prediction.

Mr. Dodd, in his discussion, has taken the data and formulæ of my paper and by handling them in an incorrect manner has deduced certain results which are directly opposite to my results. He starts with equation (18) of the paper and makes it an arbitrary substitution of $100 = a\,x$; using the resulting equation, he calculates three tables. For each of these tables the initial acceleration, the brake power and the distance are constant. By assuming more or less use of the motor curve he gets several schedule speeds, and from them calculates the energy consumption. He then plots the schedule speeds so obtained in terms of energy consumption for the three different initial accelerations. This final curve sheet, which is the summation of his paper, offers no basis for comparing the same schedule speed with different initial accelerations. All that it does show is the difference in energy required for more or less use of the motor curve, with constant power, for the three particular initial accelerations assumed. In Table A. for instance, a schedule speed of 14.02 m.p.h. is obtained, with a velocity of 165 per cent. of the rated velocity; whereas in Table B the schedule speed of 14 m.p.h. is obtained with a velocity of 112 per cent. of the rated velocity. These two values are not comparable, as they are obtained under entirely different conditions.

In addition, Mr. Dodd uses the wrong equation to calculate the input. If any equation of this character is to be used, the equation (23) is the correct one and not (18).

The misunderstanding of the purport of the paper shown in the greater part of the discussion is in striking contrast to the clear view of its scope shown by our President. His statement of its bearing on the general question could not have been improved upon.

[Communicated after Adjournment by Mr. S. T. Dodd.]

I had not intended to participate further in the discussion of Dr. Hutchinson's paper, but some of the statements in his revised reply to my original discussion demand a little further attention. Dr. Hutchinson agrees that I have "deduced results which are directly opposite to" his results, and as in his endeavors to explain that fact he falls back on the plea that I have handled his data and formulæ in an "incorrect manner," I wish to reply to his criticism.

His first criticism is that I have made an "arbitrary substitution of $\beta\,100 = ax$." I reply that this substitution is perfectly

justified by the definitions of his paper and by the data of my problem. From curve sheet 5 of the paper, the speed at full load is determined by the point $X = 100$, $Y = 100$, and by the equation "Velocity = Y in m.p.h." Therefore, on my assumptions, for the case $a = 2$, $100 = 15$ or $\beta = .15$. In obtaining a value for β in the other cases, I make use of equation (18). The equation in question is

$$P = 0.182\ (a+b)\ ax\ kw \qquad (18)$$

Where according to the definitions of the paper,

P = power per ton developed at the car axle.

a = initial acceleration.

b = coasting retardation.

x = time, in seconds during which the acceleration "a" has been acting.

ax = velocity in miles per hour at the end of the time x.

In getting an expression for the power developed by the motors at their full load, I assume x is the time necessary to accelerate the load up to full load speed of the motors, *i. e.*, up to the instant when the rheostat is cut out and the motors begin to work on the motor curve. Therefore, ax is the full load speed of the motors, or 15 m.p.h. The equation "$\beta\ 100 = ax$" is not true as a general equation, and I have not used it as such; but at the instant under consideration, when the motors are developing their rated horse power, the identity $ax = \beta\ 100$ is simply a mathematical statement of my assumption that the speed (ax), at the instant after the initial period of acceleration is completed, is equal to the full load speed of the motors (β 100), and I have a perfect right to substitute one of these quantities for the other.

The values of β thus derived, substituted in curve sheets 6, 7 and 8 of Dr. Hutchinson's paper, give me the speed, distance traversed and energy expended per ton, tabulated in my tables A, B and C, and summarized in the curves of my Fig. 4, for the accelerations $a = 1$, $a = 2$, and $a = 3$. Stated as a practical problem, I have only assumed that it is possible to gear the same equipment for a higher or lower initial acceleration with the same full load current of the motors, and I have derived from the formulæ of the paper the characteristics of operation corresponding to the accelerations $a = 1$, $a = 2$, $a = 3$.

Dr. Hutchinson is guilty of another error when he says I have "used the wrong equation to calculate the input, the equation (23) is the correct one." If he had noted my words, he would have seen that I have nowhere used equation (18) to calculate the *input*, but have uniformly used it, as defined in his paper, to express the power *developed by the motors* at the car axle. It is, however, absolutely immaterial to me what equation I use to obtain the values of β corresponding to various values of a. Any equation in his paper expressing the relation between these two quantities would have served my purpose as well. Suppose we use equation (23).

$$P = .213\ (a+b)\ \beta\ x \qquad (23)$$

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The coefficient differs from that in (18) by the factor .85, expressing the efficiency of the motors, but the relation of β to a is exactly the same as was derived from equation (18), and for the same equipment I can derive the same values of β corresponding to different values of a as I have already derived from equation (18).

I wish to criticize in this connection Dr. Hutchinson's use of P in (23) as expressing *input* to the motors while the same symbol is used in (18) to express a different quantity, the power *developed* by the motors.

I maintain that the above review of my discussion effectually disposes of Dr. Hutchinson's statements that I have incorrectly handled his formulas.

His only remaining criticism is in regard to my interpretation of the results summarized in the curves of my Fig. 4. While he admits that the curves show a maximum energy consumption with a minimum acceleration at the same schedule speed, he claims that "these values are not comparable as they are obtained under entirely different conditions." On the contrary, I maintain that the conditions are absolutely the same. We have assumed the same equipments, taking the same starting currents, the same length of run, and have arrived at curves showing the energy used for making the same schedule speed with different initial accelerations. He says my results only "show the difference in energy required for more or less use of the motor curve." I am perfectly aware that the increased economy is due to increased use of the motor curve. I cannot conceive of a variation in the initial acceleration which does not necessarily involve a corresponding variation in the use of the motor curve, the power of the equipment and the schedule speed being maintained constant. This, I have always considered, is the secret of the economy of high acceleration. If, however, Dr. Hutchinson has "elaborated a method that gives the effect of varying the initial acceleration on the energy and motor capacity" and by which he can maintain the same amount of use of the motor curve while he varies the initial acceleration, and still make the same schedule speed over the same length of run, he has failed to develop that phase of his investigation very clearly in his paper.

In closing, I wish to say that I do not wish to be understood as making any criticism on the general conclusion of his paper, in which he states: "The best initial acceleration to adopt is that acceleration giving the minimum motor capacity." Of course, it is understood that the words "for a given schedule speed" are tacitly assumed in this conclusion. With that correction, that is a statement which it did not require the array of figures Dr. Hutchinson has marshalled, to command the assent of every railroad engineer, whether practical or otherwise.

What I do criticize is his further conclusion that the lowest acceleration is the most economical, or, as he stated in his paper:

"The result is always that the energy used per mile is less for the lower accelerations." It was this conclusion that I challenged in my discussion, and I maintain that by applying the formulæ and curves of his own paper to a particular case, such as would be met with in ordinary operation, I have proved that at least this is false as a general conclusion and is not of universal applicability.

[COMMUNICATED AFTER ADJOURNMENT BY MR. W. C. GOTSHALL.]

Although I have already called attention to the action of Dr. Hutchinson in making a number of assumptions in order to arrive at erroneous conclusions and results, I cannot allow his last assertion, to the effect that I have not read his paper, to remain in the record unchallenged.

I read Dr. Hutchinson's paper a number of times, as did also my associate, Mr. Mailloux, who devoted many days in trying to adapt Dr. Hutchinson's results and conclusions to what we knew to be correct. We are just now in search of any and all data and information upon heavy and high speed electric traction, and have not been able to apply successfully the results, or the methods of Dr. Hutchinson, in our professional work, and we believe that his conclusions are in error. For instance, the run-sheets referred to in my discussion are the results obtained by taking into consideration all the known factors, and they clearly refute the conclusion of Dr. Hutchinson that the economy due to rapid acceleration is insignificant.

The first reading of his paper was followed by a feeling of perplexity, which has been augmented by Dr. Hutchinson's statements in his reply to the discussion. If, as stated in his reply, the particular results and conclusions of the author are based upon assumptions selected to suit his conclusions, which, apparently, are incapable of application in practice, the paper must stand as a purely academic production, because it falls of its own weight when analyzed by reference to the only other proper criterion, to wit: Practical application.

Again, I happen to have knowledge of the New York Subway rapid transit results thus far obtained, having carefully investigated, among other matters, their express service run-sheets and the bases of their derivation. It was on account of my being fortified with such knowledge, that I referred to it in my discussion, hoping for a relevant reply from Dr. Hutchinson. He passes over the subject with the statement that the service as proposed could not be made "under the assumptions" of his paper, in view of which I can only ask: If the formulas and results of the paper are inapplicable to conditions of practice, of what use are they?

The paper is remarkable for its lack of lucidity, as well as its vagueness. Practicing engineers have little time to devote to the derivation and verification of complex formulæ, and such work is generally only done to check the results of a new treat-

ment of an important subject. Such work is rendered manifoldly arduous when a new treatment of a subject is symbolically confused. As an illustration of ambiguity and perplexity in notation, I would call attention to the double use of the Greek letter "delta." In equation (22) this letter is used in the correct classical and universally accepted manner, as a qualifying symbol denoting an increment. In equation (27), however, and in the reasoning which precedes it, the same symbol is used as a constant, denoting an area, and, at first blush, one would naturally read equation (27), to be A minus the increment of the quantity within the brackets. It is my impression (although I would not be too sure), that the author intends it to mean the *product* of the quantity enclosed in brackets by the coefficient or factor delta. In some cases, the sign of multiplication, or the sign of equality, could have been used to advantage. On page 130 one is not sure that the formula is intended to give the results in kilowatts, or that the kilowatts are to be multiplied by the other factors in the equation. In the various formulas given on page 118, the sign of multiplication is implied between the numerical factors and the written factors, such as "feet per second" or "miles per hour." Making the same assumption in the case of the statement given in the middle of page 130 would hardly be safe, however. It seems to me, that, in this case, the sign of equality should precede the term "kilowatts."

I understand discussions to be for the purpose of bringing out from the author of a paper explanations relating to any assumptions he may have made, or for clarifying any obscurities. Dr. Hutchinson's general remarks, in his reply, were an arraignment of the participants in the discussion, for venturing to indicate hypothetical assumptions made by him in arriving at erroneous conclusions. His arraignment was based upon the apparently equally unwarranted statement of his, that the participants had not read the paper. He either could not, or would not; at all events, he did not, add one iota of light to what, I hope, is a misunderstood effort.

In conclusion, I reiterate: That the results and conclusions are in error; that the paper is not *a general solution*, "*covering all cases which can arise in ordinary practice*," (Hutchinson's paper, first paragraph), and that its adaptability to any practical case is not apparent, and has not been subsequently shown by the author.

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS

NEW YORK, Feb. 14, 1902.

The 161st meeting of the INSTITUTE was held in Havemeyer Hall, Columbia University. President Steinmetz called the meeting to order at 8.45 P.M.

PRESIDENT STEINMETZ:—Gentlemen, Members of the Institute and Guests: I hereby open the 161st meeting of the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS. Before giving the floor to the speaker of the evening, I desire to make the announcement that, by the courtesy of the University authorities, the engineering department and the engineering building have been thrown open to-night to visitors, and that you will find attendants there to show and explain all interesting features to those who desire to see them.

I now call upon the speaker of the evening, Mr. P. H. Thomas, for his paper on "Static Strains in High Tension Circuits, and the Protection of Apparatus."

[Mr. Thomas read his paper. See March issue, p. 189.]

PRESIDENT STEINMETZ:—Gentlemen, we have listened to a very interesting paper, and I believe I voice your sentiment when I say that we are all very thankful to the speaker for giving us this clear exposition and experimental demonstration of the phenomena of electrostatic disturbances in high potential lines. Discussion is now in order. Perhaps Mr. Scott will say a few words regarding these phenomena, which he has undoubtedly studied very thoroughly also.

MR. C. F. SCOTT:—Mr. President, I do not care to enter into an extended discussion of the subject which has been so well presented by Mr. Thomas. There are, however, a few thoughts of a rather general nature to which we may well give a moment's attention. A purely mathematical investigation has little interest or little value, except as a mathematical exercise. On the other hand, a purely experimental investigation, which does not connect itself with the underlying theory is something of interest, but as it is independent of other things, it has not a great deal of value. When the two are combined, when theory

and experiment can go along together hand in hand, results of value and importance may be obtained. Nevertheless, unless these results have some practical import and some useful end they are not of very great value, although they may be of interest. What Mr. Thomas has presented to us combines the three things. A sound theoretical basis, intelligent and painstaking experiment and useful and important results.

I have been very much interested during the last few years while Mr. Thomas has been carrying on his work to note the steps by which he has progressed. One day he would present a formula, something that he had found in some book, or had worked out himself, and that had some bearing on the subject. A few days later he would say that he had made experiments which showed that the formula was all right and he had demonstrated it, but he had found something else that he could not understand, in his experiments, and he then proceeded with his x's and y's and the long S's, (as some persons term integral signs) to predict new things which were then tested in the laboratory. So I think we may commend the paper as being the result of the right kind of scientific and experimental work, that which uses theoretical formulæ and the conclusions of others and applies them in a new field, which is explored experimentally, and then produces useful and important results.

PRESIDENT STEINMETZ:—I now call upon Mr. Blackwell to give us the benefit of his very wide experience with high potential transmission lines, or long-distance power transmission.

MR. F. O. BLACKWELL:—I think the INSTITUTE is to be congratulated upon the beautiful exposition of high potential phenomena which Mr. Thomas has shown us. I recall some magnets in which the exciting current had to be frequently opened. A condenser in shunt across the coils absorbed the kick and eliminated the arc at the switch. The principle is much the same as that on which Mr. Thomas's experiments are based.

There are some other causes of high-tension oscillations which might be mentioned. I have noticed often in making a high potential circuit, we get a discharge over lightning arresters which does not occur in interrupting it.

The use of much reactance in series with a transmission line to force lightning into a discharge path might produce serious results. This is very marked with " Y " connected three-phase transformers on transmission lines of high capacity. I remember with this arrangement of transformers connected to an open circuited 30,000 volt line 70 miles long, that an arc jumped across transformer terminals a foot or more apart, representing a tremendously high potential. The insulation in the transformer itself was perhaps a quarter of an inch thick and it seems strange that it should not have been punctured. This brings up another point seldom considered, that is, the ability of the best insulating materials to stand instantaneous applications of high potential. We know in testing high-tension trans-

formers that they do not break down under two or three times the potential which they are designed for and yet our tests compared with lightning are of long duration. It would seem, therefore, as if the transformers built for the high voltages now used were pretty well insulated against lightning. At least, that has been my experience. Where lightning has injured such transformers it has usually been by jumping from the terminals of the transformer to the case, rather than through the windings.

Short-circuits between turns are much more frequent than grounds, and to prevent such troubles Mr. Thomas's static interrupter will be of great value.

MR. H. W. FISHER:—As impulsive rises of voltage are mostly determined by measuring the length of the spark gap, and as the ordinary methods of doing this do not give very satisfactory results, I think it is opportune that a little time be spent in discussing this very important subject. Our society has furnished us with an excellent curve of spark distances, but it has not laid down any laws for the conditions under which spark distances may be determined with accuracy and uniformity.

There is a tendency now to measure voltage by means of the sparking distances, reference being made to the AMERICAN INSTITUTE curve, and the results obtained are apt to be misleading and inaccurate on account of the varying e.m.f. curves of the testing set, and also on account of conditions under which the spark distances may vary considerably for the same voltage. I will touch briefly on this all-important subject, pointing out some of the conditions under which varying spark distances for given voltages occur, and also mentioning conditions under which the spark distances are much more uniform.

About twelve years ago, realizing the important bearing that this subject had on high voltage testing of underground cables and the determination of disruptive rises of voltage, I began a great variety of tests which have been continued at intervals ever since.

I found that while for e.m.f.'s of 1,000 volts or thereabouts that the sparking distance was longer between very sharp points; this was not necessarily the case when the e.m.f. reached 6,000 volts or over, and for 1,000 volts between rounded surfaces I found that the spark distances might be as small as .002 inch, while between the sharpest needle points the distance became .0045 inch, the distances for blunter points lying between these two limits.

By means of a microscope and a grating having 100 lines to the millimetre, I measured and classified the diameters of the points of the sharpest needles obtainable and found that the sparking distances would hardly vary 1-1000th of an inch between points of the same diameter.

By means of electrolytic methods, I made metallic points which were still sharp under a magnifying power of 600 diameters and between points of this character the sparking distance

was about .055 inch. It is generally found that the sparking distance curve for e.m.f.'s running up to 10,000 or 15,000 volts is practically a straight line above 3,000 volts, and that this straight line does not pass through the origin but is slightly on the voltage line side of it. This apparently means that a considerable voltage is required to spark through a very small distance, and the reason for this is on account of the resistance offered by condensed air, a film of which is supposed to be on all surfaces. For the lower voltages, therefore, the spark distance curve is no longer a straight line, but it approximates more nearly to one, the sharper are the points used, and I firmly believe that if we could use mathematical points, the spark distance curve would be a straight line passing through the origin. With the points which I constructed I came within .002 or .003 inch at 1,000 volts of reaching the straight line passing through the origin to which I referred above. An explanation of the above would seem to lie in the fact that an infinitely sharp point could contain no condensed air and hence the resistance to an electric discharge would be reduced to a minimum. Based on these facts, it is evident that in measuring low voltages sharp and uniform points should be used.

With 6,000 volts I found that the spark distance could be increased as much as 25% by filing a very little off the point. Whether this action momentarily removes the condensed air from the surface of the points and thus increases the spark distance, or whether for such voltages a definite radius of curvature or form of point gives a maximum spark distance I cannot tell, but I am inclined to the latter belief.

On account of the irregular results which therefore may be obtained by slightly abraded points, it is essential that for all voltages we should use uniform points, and I have always obtained the best results with very sharp points.

Having thus spoken briefly on the effects of different kinds of points, I now come to what I consider one of the most important things that lead to discrepancies and inaccurate results, viz: Brush discharges tending in an outward direction which take place between needle points. I have studied these discharges a great deal, and I believe that they are caused by surges which are not sufficiently great to affect the average voltage.

When needles are subjected to a high voltage, the points become luminous and suddenly very small discharges may be noticed to begin in a slightly outward direction. The points of these discharges begin to move back and forth as though affected by resonance and at the same time they reach out further and further toward each other from opposite needle points. If the points of the discharges happen to come together, an arc will immediately take place between the needle points. If they do not come together they may become less and less, and finally disappear, to reappear a moment later. These discharges are the cause of great irregularity in measuring spark distances and

for e.m.f.'s of 40,000 or 50,000 volts I have obtained as much as one-half inch difference in the spark length for the same measured e.m.f., the tests being made one after the other and with needle points the diameters of which had been measured and classified. In order to get more nearly uniform results, I devised a plan to use metallic disks slightly at the back of the points, the idea being that by this method the points would be shielded, a tendency to brush discharge occurring at the edges of the disks. Under these conditions I obtained much more uniform results but unfortunately on account of the construction of my sparking apparatus I was unable to use disks greater in diameter than two inches and I believe for 40,000 or 50,000 volts the diameter of the disks should be 5 inches or 6 inches; however, at 40,000 volts the results which I obtained were much more nearly uniform than before, and for 20,000 they were extremely uniform. I believe that by some such plan as this, maximum voltages could be determined very accurately by means of the spark distance, and I would very much like to see the INSTITUTE take up this matter fully and publish methods which can be adopted over the country at large and which can be relied upon to give excellent results. I expect to continue my experiments, and hope to be able to contribute something more at a later date.

PRESIDENT STEINMETZ:—Gentlemen, the only statement I can make is to relate some experience I had with these phenomena. Last summer I had occasion to make some experimental investigations on a long-distance transmission line of very high potential at Kalamazoo. I had at my disposition two three-phase lines of 22 miles length, which doubled up gave a total effective length of 44 miles, using for half the distance two wires in multiple. The measured resistance of the line was 140 ohms; the inductive reactance at 60 cycles was 220 ohms (calculated); and the capacity reactance at 60 cycles was 6,000 ohms. The reason for choosing this line was that while operating normally at 25,000 volts, it was unusually well insulated, with line wires a distance of 42 inches apart, so that I felt confident that by series-connecting the step-up transformers I could safely operate this line at a potential of 40,000 volts and still have sufficient margin not to break down under electrostatic stress, and indeed by oscillograph I observed instantaneous peaks of potential exceeding 120,000 volts without causing a breakdown of the line.

The source of power was a turbine-driven 1,500 k.w. alternator with considerable overload capacity. As load, a combination of water rheostat and specially constructed high potential reactive coil were used, which permitted me to experiment with power factors below 40 per cent.

The primary object of the investigation was to determine the behavior of different types of switches, as enclosed oil switches, open tank oil switches, constrained air switches and open air switches, with special reference to the question whether and

what electrostatic disturbances were caused in a circuit of high capacity by rupturing it under heavy inductive load. For this purpose, the circuit was, under load, disconnected from the generating system while shunted by spark gaps, and also by the oscillograph, which recorded the variations of potential in the circuit photographically.

From the constants of the line and the load, the frequency of oscillation was calculated and this calculated frequency was found to check very closely with the frequency of oscillation recorded by the oscillograph, 240 cycles and 140 cycles respectively, with two different loads. Even the attenuation of the oscillation in the oscillograph record agreed well with the resistance of the circuits. It is interesting to note here the low frequency of the circuit oscillation. There may be electrostatic disturbance extending over a short space only, as a few layers of a transformer, and therefore of very high frequency, and also correspondingly small power, but the frequency of oscillation of the total system may be so low and near in magnitude to the normal machine frequency that it becomes a very difficult problem to guard against it.

When breaking the circuit with an oil switch, an oscillation was observed starting with an amplitude of about three-quarters of the line potential, and decreasing gradually to nothing in a number of half waves, therefore harmless to the circuit. With the constrained air switch, that is, a switch breaking the circuit at 40,000 volts in the interior of an enclosing tube, the same harmless oscillation appeared, but traces of it were already observable before the final break of the machine current. When, however, this air switch failed to open, and held an arc, the oscillograph recorded an entirely different phenomenon. Instead of the smooth sinusoidal potential wave of the circuit, it recorded the double-peaked wave characteristic of the electric arc, and this potential wave was interspersed with a number of oscillations starting with peaks as high as 115,000 volts, or over twice the normal potential.

Attempts to open the circuit with an open air switch, consisting of two metal rods 5 feet in length, separated by 10 inches at the lower end and flaring away from each other to a distance of 6 feet at the upper end, always failed at 40,000 volts and heavily inductive load, and the arc held. It flared up and stretched out to a length of some 35 feet, and wrapped itself around a pole and climbed up, reaching a line insulator on top of the pole, thereby short-circuiting the system. The short-circuit resulted in discharges and short-circuits of all the spark gaps on the system, due to the excessive rise of voltage incident to a short-circuiting arc, and a brilliant display of fireworks. The brilliancy of the arc was such that a fair photograph of it was taken at noon against a cloudless summer sky.

Especially interesting were the results observed when connecting the generator to the line while open at the receiving end. At

40,000 volts, any spark gap which I considered safe for the line short-circuited, and the oscillograph connected across the line only recorded one or two peaks of voltage reaching as high as 125,000 volts. By lowering the voltage to 30,000, I obtained an oscillograph record showing the starting oscillation superimposed upon the normal wave of the line potential. But while from the line constants a frequency of over 300 cycles is calculated for the oscillation of the line, the photographic record gave a frequency of oscillation between 85 and 90 cycles, that is, quite near the normal line frequency. This frequency explains the unusual viciousness of the starting oscillation. It also shows, however, that the oscillation produced in the line by connecting it to the generating system was not limited to the line only, but extended back into the generating system through step-up transformers into the low potential generator. A rough approximation of the frequency to be expected from the line capacity and the total inductance of line, step-up transformers and generator, fairly agrees with the observed frequency. Oscillations, therefore, may extend over the whole system through transformers and into generators, and an oscillation produced by a disturbance in one line or cable may reach any other line or cable connected to the same generating system, even when fed by separate step-up transformers. This explains the repeatedly observed fact that a breakdown in one circuit or cable causes successive breakdowns or short-circuits in other cables, etc., connected to the same generating system, by the oscillation reaching back to the generator bus-bars and into the generator.

The conclusions which I derive herefrom are that the danger of breakdowns by high potential oscillations is far greater than frequently assumed, and that the most dangerous oscillations are not the very localized oscillations of very high frequency, but oscillations of relatively low frequency extending over the whole or a large part of the system, and due to changes of the circuit conditions, as short-circuits, etc., since they are so low in frequency and so near to the machine frequency that no reactive coil can guard against them without being so large as seriously to impede the normal generator current, and that to protect against these oscillations is probably the most serious problem to be met with in the establishment, construction and operation of high potential systems.

MR. PHILIPPO TORCHIO:—I would like to ask Mr. Thomas if in a 6,600-volt three-phase 25-cycle underground system, as operated in this city, an equipment of spark arresters and static interrupters would be considered necessary or proper.

MR. THOMAS:—That is largely a question of the particular design of the apparatus that is to be protected. On 6,300 volts, with transformers, the concentration of potential, such as the static-interrupter is intended to prevent, would not be a serious matter, because there is margin enough allowed in the design of the apparatus. In the case of a high potential generator, how-

ever, which has a great number of turns of wire, and in which it is impracticable to obtain a large amount of insulation, it is entirely possible that if trouble has been experenced in any case, a static interrupter would help very much. A static interrupter (I repeat for the benefit of those who have not noticed this point), has no direct protective power against the grounds; it simply prevents the concentration of potential on a few turns of a coil, and hence is no especial help in cables. The lightning arrester gives protection against grounds. On the 6,300-volt circuits, we probably would need some protection against grounding at the end of cables, possibly at some other point. But unless the generator require protection, I do not see that the static-interrupter would be of any value. The static-interrupter is of special use on high voltages—for example, those of 20,000, 30,000, or 40,000 volts.

MR. TORCHIO:—There is another question which I would like to ask. In a 6,600-volt system, the question of protecting the apparatus itself against electrostatic disturbances is of secondary importance, as the apparatus is designed with a large factor of safety even for very high voltages of an oscillatory nature. In an underground cable system, however, insulation conditions may be poor at several points, as at joints, etc. Now, if a ground develops on one leg of a feeder and at the same time there happens to be a weak spot on another leg of another feeder connected to the same bus-bars, the doubling of potential on this weak spot will break down the insulation and start a short-circuit through ground. Now, if the center of the "Y" connection of the generator were permanently grounded, the grounding of any one leg of the system would cause immediately a short-circuit, against which the cable is well protected by the automatic circuit breakers at the station, but the strain of double potential would be removed from the system. I would like to have Mr. Thomas's opinion on this subject.

MR. THOMAS:—I feel about the question of neutral grounding, that on very high voltages it is probably the best method of operation. Of course, this would depend somewhat upon circumstances. On a system in a large city which has a comparatively low voltage, the desirability of grounding the neutral would depend on the conditions. Certain strains are made less by the grounding of the neutral point. Certain other strains are made greater. There are certain advantages, and it should be determined by the circumstances in any particular case, or perhaps by actual experience, which is the better method of operation. I think this cannot be determined off-hand.

As to resonance, there may be two kinds of resonance; static resonance, that is, resonance in which the exciting alternating force, the force which is necessary for producing resonance, is of a temporary nature, such as some secondary disturbance, like a lightning discharge or breakdown of some kind, which has a limited amount of power and a limited duration; and second,

resonance in which the exciting force is the generator. In the first case, a discharge gap to ground may very well be a sufficient protection, because as soon as the voltage rises, this gap will discharge. This will relieve the potential and even if it builds up once or twice more by resonance, the spark gap will discharge again. When the generator supplies the exciting e.m.f., however, there is very much power behind it, and the resonance condition will continue indefinitely; so that a spark gap which will prevent a rise of potential must be able to carry current continuously. Of course such a gap is pretty nearly out of the question; this case would be equivalent to a short-circuit.

PRESIDENT STEINMETZ:—Gentlemen, before adjournment I desire to announce that the next meeting will be held on Feb. 28. The subject will be "Electrochemistry and Electro-Metallurgy." Very interesting papers are promised by Dr. Sheldon, Mr. Bradley, Dr. Keith, Mr. Hering, Mr. Jacobs and others.

The meeting then adjourned.

[COMMUNICATED AFTER ADJOURNMENT BY MR. B. A. BEHREND.]

Mr. Thomas's paper is an extremely instructive and timely summary of some of the phenomena that have obtained prominence in recent years with the growth and development of high potential apparatus. I wish to illustrate some points made by Mr. Thomas by two diagrams which show very clearly the immense strain which has to be sustained by a cable, a transformer or a synchronous or induction motor armature in the moment at which it is put on to the high potential mains. The mathematical analysis from which these curves have been obtained, though somewhat complex, is identical with the theory of the linear movement of heat as given in Fourier's classical work on the "Analytical Theory of Heat." Lord Kelvin in 1855 applied Fourier's analysis to the case of a telegraphic cable in his masterly papers on the "Electric Telegraph" to which I wish to refer, as these researches have a distinct bearing on the phenomena presented by high potential discharges. If we connect 1,000 volts direct current to 10 incandescent lamps in series, by suddenly closing a switch, very often the lamps near the terminals break down, because in the first instant there is a greater potential across them than across the lamps near the middle between the two terminals. This occurs only in the first instant, as after a very short time the potentials across the lamps become equal.

Let us imagine a long cable, one end of which is suddenly raised to the potential 1,000 volts, then at successive intervals of time the potential distribution will be represented by the curves numbered 1, 2, 3, 4, 5, 6, 7, 8, 16 and 256 in Fig. 1.

The general partial differential equation from the integral of which the curves in Fig. 1 are derived is Fourier's well-known fundamental equation.

$$\frac{d^2 v}{dx^2} = C R \frac{d v}{d t} \qquad (1)$$

C being capacity per unit length and R resistance per unit length, the solution of this equation for a cable of length l is expressed by

$$v = V\,(1 - x/l)\left\{1 - \Sigma A_i \sin\left(\pi \frac{i\,x}{l}\right)\varepsilon^{-\frac{i^2\pi^2 t}{RCl^2}}\right\} \qquad (2)$$

In order to satisfy the equation (1) $\Sigma A_i \sin[\pi\,(i\,x/l)]$ must be equal to the final value of v, i. e., $(1 - x/)\,lV$, by which condition the Fourier series, and hence the problem, becomes determinate. Now Fourier and Lord Kelvin have shown that this integral may be transformed into the well-known probability integral which is found evaluated in treatises on the theory of probability. Thus (2) becomes, for an infinitely, or very long cable,

$$v = V\left\{1 - \frac{2}{\sqrt{\pi}}\int_0^z \varepsilon^{-z^2}\,dz\right\} \qquad (3)$$

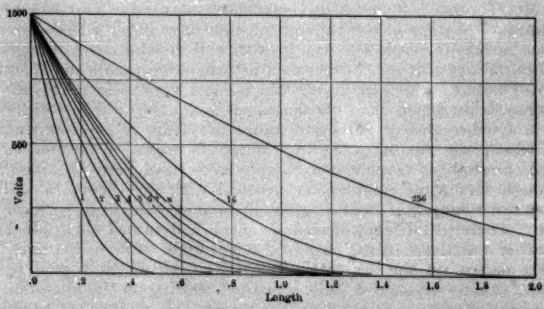


FIG. 1.—Distribution of Potential in a Cable at Different Times and at Different Points.

in which equation z is equal to $\frac{x\sqrt{CR}}{2\sqrt{t}}$. It follows directly from the limits of the integral (3) that if t is taken proportional to the square of x, the integral remains constant. Hence, curve 16 being plotted from the table, the curves of the distribution of potential at different times may be found by inserting the times for which we wish to calculate the integral into $\frac{x\sqrt{CR}}{2\sqrt{t}}$ and changing x so that the fraction remains constant. Thus we obtain the curves in Fig. 1. Lord Kelvin in his paper on "Fourier's Law of Diffusion" plotted similar

curves, but he did not plot them for successive time intervals as I have done, probably because this is more wearisome.

It is also easy to transform the Fourier integral (2) into an integral which permits of evaluation by means of a table, but I must reserve this for another occasion. This would then give the conditions for a cable of length l.

The strain which the insulation has to sustain is expressed by

$$S = - K \frac{\partial v}{\partial x} \tag{4}$$

hence the differentiation of the curves of Fig. 1 must give the strain in the cable at different times and at different points. I have carefully worked out these curves for my own satisfaction with the help of the table of the values of the probability integral, and plotted them in Fig. 2. The ordinates of these curves

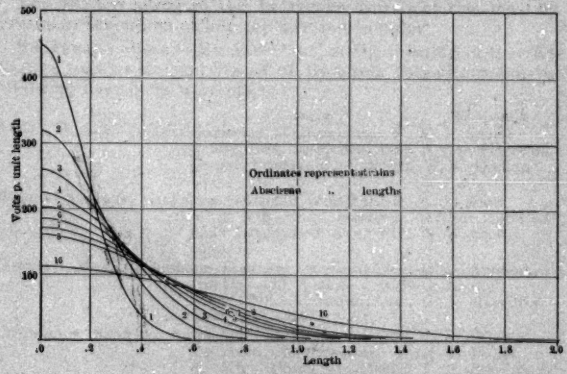


FIG. 2.—Diagram Illustrating Insulation Strains in a Cable at Different Times and at Different Points.

represent directly the strain in the insulation at different times and at different points, and it will be seen at a glance how severely the insulation near the terminal is strained at the moment of putting it in connection with the line.

A mechanical analogy will help us to understand the diagram Fig. 2. Assume the cable to be similar to a rubber hose which is suddenly put in connection with water under pressure, then the hose will stretch, *i. e.*, it will enlarge its diameter in proportion to the ordinates of the curves marked 1, 2, 3, etc., *i. e.*, it will assume at successive moments the shapes represented by these curves, and it is plain that, unless the rubber is very strong, the hose will burst at one end the moment it is put in connection

with the water under pressure. Our equations neglect the self-induction and are therefore only approximate, but as they permit a very clear insight into the effect of time on the distribution of potential, no attempt has been made to be more rigorous. In our analogy with the hose we should have to assume the water to be without mass, but having considerable friction along the walls of the hose.

If the transformer is put with one terminal on to the line at the moment of maximum e.m.f., these curves show approximately the conditions prevailing in the transformer as the speed with which the electric wave penetrates is large in comparison with the frequency.

Cincinnati, August, 1902.

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

NEW YORK, Feb. 28, 1902.

The 162d meeting of the INSTITUTE was held this evening, President Steinmetz calling the meeting to order.

SECRETARY POPE:—Mr. President, at the meeting of the Executive Committee and Board of Directors this afternoon, the following associates were elected:

Name.	Address.	Endorsed by
AYRES, ALBERT DOANE,	Salesman, Westinghouse, E., & Mfg. Co.; residence, 3808 Grand Boulevard, Chicago, Ill.	C. F. Scott. Arthur Hartwell. W. K. Dunlap.
BURTON, CHARLES GILLETTE,	Salesman, Westinghouse E. & Mfg. Co.; residence, 6328 Greenwood Ave., Chicago, Ill.	C. F. Scott. Arthur Hartwell. N. W. Storer.
CHESTER, M. E.,	Telephone Engineer, Western Electric Co., 463 West St.; residence, 296 Manhattan Ave., New York City.	G. F. Atwood. O. A. Bell. G. A. Hamilton.
CHUBBUCK, LEONARD BURROWS,	Checker in draughting room, Westinghouse E. & Mfg. Co.; residence, (Homewood) 535 Clawson St., Pittsburg, Pa.	C. E. Skinner. H. P. Davis. T. R. Rosebrugh.
COOPER, WILLIAM,	General Superintendent, Bullock Electric Mfg. Co., Cincinnati, Ohio.	R. T. E. Lozier. B. A. Behrend. L. E. Bogen.
COWEN, JULIAN BETTY,	Manager of Export Department, General Incandescent Arc Light Co., 572 First Ave.; residence, 204 W. 119th St., New York City.	Samuel Insull. M. J. Insull. H. P. Ball.
DRYER, ERWIN,	Salesman and Engineer, Westinghouse E. & Mfg. Co.; residence, 171 La Salle St., Chicago, Ill.	C. F. Scott. Arthur Hartwell. N. W. Storer.
ELEY, JOSIAH NORFLEET,	Electrician and Operating Engineer, Georgia Electric Light Co., 198 S. Prior St., Atlanta, Ga.	C. N. Black. W. E. Moore. J. P. Edwards.

FERRIS, ROBERT MURRAY, JR.,	Engineering Department, The N. Y. & N. J. Telephone Co., 81 Willoughby St., Brooklyn, N. Y.; residence, 239 West 103d St., New York City.	J. C. Reilly. J. J. Carty. B. Gherardi, Jr.
FULLER, EDWIN ERNEST,	Representative Engineer for Scotland for The British Thomson, Houston Co.; residence, Abbottsford, St. Andrews St., Ayr, N. B., England.	W. J. Clark. A. Churchill. H. S. Meyer.
GAYLORD, TRUMAN PENFIELD,	Engineer, Westinghouse Mfg. Co.; residence, 610 Dearborn Ave., Chicago, Ill.	C. F. Scott. Arthur Hartwell. N. W. Storer.
GIBSON, JOHN JAMESON,	Salesman Westinghouse E. & Mfg. Co.; residence, 171 La Salle St., Chicago, Ill.	C. F. Scott. Calvert Townley. F. A. Pickernell.
GREEN, CHARLES MAXWELL,	Engineer on Brush Arc Dynamos, General Electric Co.; residence, 85 N. Common St., Lynn, Mass.	E. Thomson. C. P. Steinmetz. P. G. Gossler.
KINTNER, CHARLES JACOB,	Solicitor of Patents and Expert, 45 Broadway, residence, 36 E. 29th St., New York City.	R. W. Pope. C. W. Price. T. C. Martin.
KINTNER, SAMUEL MONTGOMERY,	Professor of Electrical Engineering, Western University of Pa.; residence, 1916 Perrysville Ave., Allegheny, Pa.	C. F. Scott. R. A. Fessenden. H. W. Fisher.
LLOYD, EDWARD WILLIAM,	Assistant Superintendent of Construction, Chicago Edison Co., 139 Adams St., Chicago.	L. A. Ferguson. James Lyman. W. G. Carlton.
MILLER, GEORGE E.,	Assistant to Fourth Vice-President, Westinghouse E. & M. Co.; residence, 4911 Centre Ave., Pittsburg, Pa.	R. W. Pope. H. B. Shaw. C. F. Scott.
MORRILL, EDWARD FRANCIS,	Engineering Department, The New York and New Jersey Telephone Co., 81 Willoughby St.; residence, 18 Willoughby Ave., Brooklyn, N. Y.	B. Gherardi, Jr. F. H. Dassori. Samuel Sheldon.
MULLIGAN, WALTER LYON,	Assistant Manager, United Electric Light Co., Box 812 Springfield, Mass.	Joseph Sachs. R. W. Pope. G. W. Blodgett.
LOWSON, DAVID,	Electrical Inspector, Department of Electricity, 1721 Park Row Building; residence, 35 W. 132d St., New York City.	A. A. Nimis. Joseph Sachs. R. W. Pope.
MARBURY, LOUIS C.,	Electrical Engineer, General Electric Co., Schenectady, N. Y.	C. P. Steinmetz. W. R. Whitney. A. H. Armstrong.
TER MEER, HENRY CHARLES,	Tester in Meter Department, Brooklyn Edison Co.; residence, 990 Hudson St., Hoboken, N. J.	F. B. Crocker. M. I. Pupin. G. F. Sever.

NICHOLS, LOUIS CHARLES, Electrical Engineer, The Converse Transformer Co.; residence, 602 Shady Ave., Pittsburg, Pa.	H. B. Smith. V. G. Converse. A. L. Rohrer.
PRICE, NORMAN I., Sibley College, Cornell University; residence, 209 Huestis St., Ithaca, N. Y.	G. L. Hoxie. H. J. Ryan. E. L. Nichols.
REMSCHEL, CESAR WILHELM AUGUST G. P. O., San Francisco, Cal.	C. O. Poole. F. E. Smith. C. W. Waller.
RUFFER, CHARLES SHUMWAY, Electrician, The Telluride Power Co., Telluride; residence, Vance Junction, Col.	H. B. Shaw. P. N. Nunn. W. K. Freudenberger.
SAWYER, WILLITS HERBERT, Engineer, Railway Engineering Department, General Electric Co.; residence, 4 Gillespie St., Schenectady, N. Y.	C. P. Steinmetz. W. B. Potter. W. I. Slichter.
SHARP, CLAYTON HALSEY, Test Officer, Lamp Testing Bureau, 14 Jay St., New York City; residence, 722 Highland Ave., Newark, N. J.	W. S. Howell. A. E. Kennelly. C. W. Rice.
SLOAN, JAMES RICHARD, Electrical Engineer, The Pullman Co., P. R. R. Depot, Jersey City, N. J.; residence, 607 West 61st Place, Chicago, Ill.	W. M. Stine. W. L. Bliss. A. W. Berresford.
THOMAS, ALFRED CLARENCE, Engineer, The New York and New Jersey Telephone Co., 81 Willoughby St.; residence, 131 DeKalb Ave., Brooklyn, N. Y.	J. C. Reilly. J. J. Carty. B. Gherardi, Jr.
WAGONER, PHILIP DAKIN, Commercial Department, General Electric Co., Box 728, Schenectady, N. Y.	C. P. Steinmetz. W. C. Woodward. C. D. Haskins.
WILKINSON, JAMES, Chief Engineer Birmingham Railway, Light and Power Co.; residence, 1212 North 17th St., Birmingham, Ala.	G. H. Harris. F. E. Kinsman. R. W. Pope.
WOODFIELD, SYDNEY, Assistant Engineer, Electrical Power Distribution Co., York House, Norfolk St.; residence, 24 Sinclair Gardens, Kensington, London, England.	C. W. G. Little. R. W. Pope. F. E. Kinsman.

Total, 34.

The following associates were transferred to full membership:

HOWARD S. WILSON, Superintendent, Puebla Electric Light Co., Puebla, Mex.

CHRISTOPHER M. GODDARD, Secretary, Underwriters' National Electric Association, Boston, Mass.

BYRON T. BURT, Superintendent, Chattanooga Electric Light and Power Co., Chattanooga, Tenn.

LEWIS A. HOWLAND, Engineering Department, Canadian General Electric Co., Montreal, P. Q.

MR. CALVIN W. RICE:—Mr. President, it may be of interest to the INSTITUTE to notice that at the luncheon given in honor of Prince Henry, of the one hundred "Captains of Industry" invited, there were our President, Mr. Steinmetz, five former Presidents of the INSTITUTE, Messrs. Thomson, Bell, Sprague, Weston and Crocker, and six other members, Messrs. Edison, Tesla, Brush, Pupin, Acheson and Morton. Of those who are not members, but eminent in electrical interests, there were Mr. George Westinghouse and Mr. Vreeland.

PRESIDENT STEINMETZ:—Gentlemen: the topic of to-night's meeting is Electrochemistry, a branch of electrical engineering which, when this INSTITUTE was founded, and even ten years ago, was hardly existing or in its very beginning, while now a very large part of the country's water-powers are utilized in this branch of the electrical industry.

Many commodities, well-known to us from old times, such as copper, etc., are now produced electrolytically in large quantities, while materials which ten years ago were mere chemical curiosities are now in everyday use, as aluminium, calcium carbide, etc., and substances as carborundum, unknown heretofore have been introduced into the industries and arts by being produced by electrical means.

The electrochemical industry is based upon two features of the electric current—the electrolytic action and the electrothermal action. For electrolysis, electricity can be used only as continuous current, or at least is so used thus far. For electrolytical dissociation of chemical compounds, only very low voltages are required, since the chemical union of atoms is dissolved by very low electric force, a few volts at the most. The result is that the electrolytical industry employs very large currents at very low voltage, and relatively low, even when series connection of very many electrolytic cells is employed.

The second use of the electric current for chemical work is based on the heating effect of the current. Here the electric current has no direct chemical action, and is merely a means of producing heat in the electric furnace.

We can distinguish two types of electric furnaces, the arc furnace, where the heat of the electric arc is utilized, and the incandescent furnace, where the electric current passes through very refractory resistances and thereby produces heat, and both types, while to a certain extent interchangeable, have some different characteristics.

The arc furnace is necessarily the more powerful. The temperature is the highest which has ever been reached by artificial means, a temperature at which chemistry almost ceases to exist. There are very few chemical compounds which still exist at that temperature. Almost all chemical unions have been dissolved there.

In the incandescent furnace any temperature may be reached up to the temperature where the material of the furnace disintegrates or evaporates, boils or melts down.

The electric furnace thereby gives us a very useful means to reach temperatures beyond those which can be reached by combustion; that is, temperatures beyond the maximum temperature of combustion as limited by the temperature of dissociation of the result of combustion; that is, temperatures beyond those where water, vapors and carbonic oxide dissociate in their atoms. At these high temperatures, the electric furnace is the only means of producing the temperature; below, combustion may be utilized.

No matter how efficient an electric furnace is, the heat produced from electricity necessarily must be very much more expensive than the heat produced by combustion, since by combustion virtually all the chemical energy is converted into heat, but by the electric furnace there is converted into heat only the electric energy which in turn is only a very small part of the chemical energy of the coal fired under the boilers of the engine driving the generator. Therefore, the electric furnace can find efficient application only for producing heat beyond the range available by combustion, and furthermore, to a more limited extent, below the range of temperature available by combustion in the case where a very accurate control of temperature is required, or where the ordinary means of heating are unsuitable for the particular purposes, for reasons outside that of efficiency, as deterioration of apparatus, etc.

In the electric furnace continuous current as well as alternating current may be used, since only the heating effect of the current is required. However, alternating currents are practically always used, owing to their greater facility of generation, of transmission and especially of control. So the electric furnace is virtually always an alternating furnace, while for electrolytic work continuous current is used.

Besides these two phenomena, the electrolytic action and the electrothermal action, there is still a further effect of the electric energy, that is, especially at high voltages, the direct disintegrating or combining action of electrostatic stress, as you observe in the Geissler tube, etc., and I believe now efforts are being made to utilize this for electrochemical work.

MR. J. W. LIEB, JR.:—Mr. President, before you call upon the gentlemen who are to present papers this evening, permit me to call to your attention the fact that we have with us as a guest, Mr. Maurice Leblanc, whose name is almost as well-known to the electrical industry in America as it is in Europe. I am sure that the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS is pleased to have Mr. Leblanc here, and will extend to him a hearty welcome.

PRESIDENT STEINMETZ:—I have had the pleasure already of greeting personally Mr. Leblanc, and in the name of the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS I now cordially welcome Mr. Leblanc, and give him the greeting of our institution. We hope to see him more frequently here.

Discussion.

Dr. Sheldon then presented his paper. He was followed by Mr. Charles B. Jacobs on "The Electric Furnace in Industrial Chemistry." Mr. Carl Hering presented three papers entitled respectively "Electrolytic Conduction without Electrodes," "Liquid Potentiometer—Determining Electrolytic Resistances with Direct Current Instruments," and "Point of Cut-off in a Battery Discharge." Papers were also presented by Dr. N. S. Keith on "Electrolytic Recovery of Copper from Low Grade Ores," and by Mr. W. R. Whitney on "Colloids."

President Steinmetz:—Gentlemen, we have listened to a series of very interesting papers. Before I call for the discussion, I believe that I voice your sentiments by saying that we shall all be glad to hear a few words from our distinguished guest and visitor, Mr. Leblanc.

M. Maurice Leblanc:—(*Translation.*)—Never so much as this evening have I regretted not knowing English, for I do not know how to thank you for your excellent reception.

This is the first time that I have come to America. The little that I have seen thus far has inspired me with the greatest admiration for your prodigious activity and the greatness of your undertakings, as well as with the desire to become acquainted with your country in the most complete manner.

When I return to France I shall not neglect to communicate my impressions to my colleagues and make them share in the welcome which you so kindly wished to extend to us.

I thank you once again, gentlemen, and tender my best wishes for long prosperity to the Institute.

President Steinmetz:—I now call for the discussion and should like to hear first from Mr. Bradley, who, as we all know, has given a great deal of attention and study to this subject, and has done a good deal in the field of electrochemistry.

Mr. Charles S. Bradley:—Mr. President and Gentlemen: I have been scheduled for a paper on experimentation in electrochemistry. I find very little time to devote to such a subject, but I will state to you in a few words my ideas for the conduct of this line of business, or in fact, any experimental business. My conclusions have been reached after fifteen years of study and twenty years of experience.

I would first call your attention to the theory of evolution. Although mentioned in the early ages, even in ancient Grecian literature, it was left for Darwin, Spencer, Huxley and other eminent men to spread it. It has reached the scientific people and they have applied it in all sorts of ways, but it is not yet intelligently applied in business. Undoubtedly, there are business men who appreciate the full value of it, but really the mass of business men have not yet found out that the theory of evolution applies to all kinds of business, even to their bookkeeping; if they would study they would see it.

My idea is simply the application of the theory of evolution to experimental work. We first require the man as a nucleus. Next we require a little money and a laboratory. Five hundred dollars will determine, by careful application, whether an idea is worth going ahead with or not.

When any invention is worth developing, we should carefully select the man as a nucleus, then the laboratory, and we have, then, the basis of a process. Next, we require a company—a company which may be evolved, not with the enlarged capital stock which most people seem to prefer, but with small capital. Then we require more work; then we require more capital, and so the whole thing evolves, step by step, and we need also very soon a corps of men to conduct the business. We need that much sooner than people have any idea of. No one man has the whole thing in his mind—he cannot have it and that shows the necessity of a company.

A company is a very valuable thing in a great many different ways. It gives steadiness of purpose. It brings all sorts of minds to bear upon the subject. It brings the restraint of one man upon another, and the mistake that most persons make is that they do not employ enough men. They need to employ men, when the latter apparently have nothing to do. We have men in our employ who are not really doing anything useful for the time being. They think they are. That is all right. I want them to go on thinking that they are doing something useful, and before long we will find that some one of them has done a very creditable thing—something that is worth more than all of our capital stock. We have to trust to the average of results, and it is surprising what those results are. In a few years, if not at present, we will be able to show remarkable results. If I were to take you into our confidence and show you our accounts throughout, you would say, what a success you have made since 1897. Whatever has been done was accomplished by applying the theory of evolution, and such intelligence as we could bring to bear on it, or more exactly speaking, making such selection as to produce the best results. It is very much like a tree, putting out its roots in all directions. Some of these roots run against a rock and they stop growing; others go on and draw sustenance for the tree. Of numberless experiments that are tried, out of a hundred perhaps one shows some promise. It is just the same as when the tree puts out its roots; some of the roots die off, others survive.

I hope that some day I can write this out in good shape so that it may be a lesson for the future, because the world needs now to stop and take account of stock. It needs to learn to apply the scientific facts that have been gathered in the past two or three hundred years, or in other words, to apply the knowledge of evolution, selection and survival to the ordinary affairs of life and especially to new work.

We also need the application of ethics. Your Committee on Papers has asked us to give papers here, and it has found that

some of us are loath to do so. Why? Because we do not wish to give away our business. We do not wish to tell the whole of any process which we may have. We expect that some one will go to work and become our competitor, which would injure our prospects. Therefore, we try to keep quiet.

Now, this should not be the case. We should not be afraid of anybody. The world should not impose that upon us, and if the theory of evolution was thoroughly studied, and if science was thoroughly studied, and if people would take account of stock, they would not compete. I have tried in every way in our business to avoid competing with any electrochemical industry. If our men come to me and say that they have a process for the production of caustic soda, I say, " No, sir, stop work right where you are; there is no use; there are thousands and thousands of things we can work on without working on caustic soda. There are four or five concerns trying to produce caustic soda now and we do not wish to injure them." So, we apply the knowledge we have gained to something else, and if all would do the same, we would be perfectly willing that you should come to our manufactory and look over every bit of it. If this thing were fully understood, then we might all have complete confidence in each other, and all enjoy the experience of each in his own particular line.

I would like to correct one or two statements in Dr. Sheldon's paper, on the subject of camphor. The production of camphor is one of the processes of our company. He says " But the process is claimed to be in no wise electrical." That is perfectly true. The statement he makes is this: " If turpentine hydrochloride, in a melted condition, or in an alcoholic acetic acid solution be electrolyzed, camphor is obtained." This statement is nothing to us except that it might lead somebody to go into a lot of experimenting that would be useless. The alcoholic solution of any hydrous acetic acid with pinene hydrochloride dissolved in it is a very poor conductor,and almost a non-conductor, and when water is added the specific resistance is more than 1833 ohms; that is, the resistance of a centimetre from one face to the other is 1833 ohms. This shows you that we could not get current enough through it to produce any practical results with any moderate size of apparatus.

The next statement is " Camphor results from the oxidation of terbene, *i. e.*, oil of turpentine." And then he goes on to say, " Artificial camphor is made from turpentine in large quantities by the Ampere Electro-Chemical Company."

In an experiment which was tried in the laboratory on April 7, 1900, a part of a sample of pinene hydrochloride was melted in a crucible, two electrodes introduced. Various pressures up to 80 volts were tried. The distance was then lessened until the electrodes were short-circuited beneath the surface of the liquid. It seemed to be an insulator, and it did not appear to undergo electrolysis. I think that this disposes of the application of

electricity to these things in a commercial way. However, Mr. Jacobs can take up the subject, if we have time later in the evening, and show the reason why these could not produce practical results on account of the chemical formulas.

The camphor is produced by treating turpentine with oxalic acid, which produces the pinene oxalate. This oxalate is then treated with lime or with any other of the alkalies, preferably lime, which produces the oxalate of lime and sets free borneol. This, which is distilled and caught in a condenser, is then treated with an oxidizing agent, which converts it into ordinary camphor, or the camphor of commerce. The camphor that we produce is not the artificial camphor in the sense in which that name is generally used in the market. The market calls hydrochloride of turpentine artificial camphor, but our camphor must be distinguished from this. Ours is synthetic camphor and has exactly the same properties as the camphor of commerce.. We have tried to substitute a great many substances for the oxalic acid but all fail to produce camphor.

I notice that artificial rubber has also been mentioned. This is also thoroughly chemical, nothing in the line of electrical application.

My opinion is that the chances in organic chemistry for the application of electricity will be very small. It is possible that with some of the starches or allied substances, in which there is a possibility of using water solutions, there may be a field for work —what I mean is for commercial results, where you can produce quantities enough to make it pay. Perhaps this statement might be modified, and I might say that where the price of the stuff is extremely high we might be able to produce sufficient quantities to make it pay, but wherever the price is low, this would probably be impracticable.

Referring to Mr. Hering's paper, the unipolar machine has led to more mistakes than almost anything that has ever been thought of. In fact, Prof. Crocker and myself built one once that was made in the most beautiful fashion and we ran it some 2,000 turns. It had a hundred turns of wire and it was supposed to cut the magnetic field and then the wire was carried down through slots. We hoped by going through the slots that we would dodge the magnetic lines. We tried it, and we did not get much current. We could not see that we got any. Finally, after repeated efforts, we connected it directly with a mirror galvanometer of 5,000 ohms, and it not so much as moved the needle. We could wet our fingers and touching the binding posts generate enough current to move the spot off the scale, but this machine would not generate a particle. I do not see where it is possible that Mr. Hering has completed his circuit here, unless it goes through the body of iron of the magnet. There must be a return somewhere, and if there is, he has the electrodes there just as we have in all kinds of electrolytic cells.

MR. CARL HERING:—In replying to Mr. Bradley's criticism, I

call attention to the radial channels or tubes, connecting trough A with trough B and shown in dotted lines; there is also an arrow showing the direction of the current in them. These channels are stationary, with respect to the magnetic field, hence they do not cut the lines of force, and no e.m.f. is induced in them. This return circuit of the electrolytic circuit, therefore, consists entirely of electrolytes without any electrodes; the current does not return through the metal of the magnet, as Mr. Bradley supposed, but through insulated tubes passing through the iron.

DR. C. A. DOREMUS:—Mr. President: I fully coincide with what Mr. Bradley has said as regards the evolution of chemical work. Anyone who is familiar with the chemical industries in Germany, France and England, knows exactly why they have evolved into the prodigious establishments which there exist; why it has been found feasible to have a hundred or more trained chemists constantly at work on problems which apparently have no direct influence on the business, but which in later years turn out to be most influential in the particular business in which that company is interested and which mould the policy of others.

In this country we are just beginning to appreciate the value of our raw materials. In the last twenty years our chemical laboratories have been turning out young men more or less according to American methods. It is about time that we should begin to feel their influence on our industries, and with the possibilities which electricity has brought, of being able to put heat where we want it, and current where we want it, and other actions where we want them, and as we have the water powers to generate the electricity, or the oil and the gas and the coal to generate the fire to give us the electricity, there is no reason why we should not be able to bring about wonderful transformations of matter.

No chemist is unfamiliar with the fact that there is required a definite amount of energy in every chemical reaction. He is no true disciple of Faraday if he does not know it. I think that in the next few years we will see in this country a growth which will amaze ourselves as well as other people, and it need not be in the direction of improvements on what has been possible, but, as has already been brilliantly shown in this country, in the development of new industries.

In speaking, not in criticism but in discussion, on some of the papers from a chemical standpoint, I think it is unfortunate to use the word "colloids." It is a misleading term. Prof. Whitney, in his opening remarks, speaks about the use of the word by Graham. Now, the colloids of Graham and the "suspended" particles in solution which Prof. Whitney afterwards alludes to are so entirely different in their physical properties that they ought not, in my mind, to be classed in the same group as "colloids."

If we precipitate silicic acid from a silicate solution by the use of hydrochloric acid, it re-dissolves immediately in the acid

and we get a solution which is absolutely transparent, which contains much salt and which may be passed through filter paper, but which will not pass through parchment paper.

When put in a dialyser, the salt will go through faster than silicic acid, but the silicic acid will also go through, and we call this dialysing or dialysis. If we change the water often enough we finally get a solution of silicic acid in the dialyser, which may be concentrated to about 14%. It is absolutely transparent, but will, on proper provocation, become a perfect gelatine. Such silicic acid is one of Graham's colloids.

If, on the other hand, we place a very little gold solution in a bottle of perfectly pure water and add a pinch of phosphorus dissolved in ether, the gold is precipitated in such minute particles that the liquid looks transparent and red; by reflected light it is perfectly yellow. With a lens we can trace the rays of light, which we cannot do with a silicic acid solution, where there are no suspended particles. The solution of gold will continue to settle for months. I had such a solution for over a year in the laboratory. There will be a deposit and that deposit will vary in color according to the fineness of the gold deposited, just as when we pass an electric spark from a Leyden jar through a thread of gold, we can deposit the gold on a piece of glass, gold of all colors, green, red, yellow and violet. Now, the gold "solution" is really a mass of suspended particles not colloidal, because if we place it on a filter, it will run through the filter radially but not axially, the latter collecting the gold particles. It seems to me that we ought to have very clear ideas about these two types of "solutions."

The electric influences or the electrolytic influences are not always the ones that will bring about coagulation. For instance, if we extract from the stomach of the calf a body called chymosin, an infinitesimal quantity of it, placed in warm milk, will solidify the milk so that it will not fall out if the vessel is turned upside down. The casein has become jelly almost in an instant. Chymosin is not an electrolyte. This is a very different thing from putting a little salt into the gold "solution" where the gold is suspended.

When we discuss theoretical matters, the use of words is a matter of very considerable importance. Chemists speak of a dialysed iron solution, which is hydrate of iron in a perfectly soluble transparent form, which is a "hydrosol" condition, but if we convert that into precipitated iron hydrate, it has the "hydrogel" form. We have alumina in just these conditions. We can dialyse aluminum salts and get aluminium hydrate in the hydrosol condition, and it will coagulate into a hydrogel form. We can convert aluminium hydrate in an alkaline solution into the dialysed form, or from an acid solution into the dialysed form. In one case we get a hydrogel form that will unite with coloring matters and in the other it will not. Are these conditions electrical?

DR. N. S. KEITH:—Mr. President: while commending Dr. Sheldon's paper, I desire to supplement it with a few considerations.

The actions and reactions in both battery and electrolytic cells are analogous; and are those of analysis and synthesis. If these be not reciprocal, the counter-electromotive force of polarization is set up in a battery, as in an electrolytic cell, wherein the oxygen and hydrogen of water are dissociated. In a deposition cell containing a suitable cupric sulphate solution, with soluble copper anode and a cathode, already having a coating of copper, unless the current be made too dense, the copper is dissolved from the anode and deposited on the cathode in equal amounts, and there exists no other resistance to the passage of current than those of the electrolyte and conductors.

Right here let me say that we need some distinctive terms with which to designate three kinds of cells in which chemical actions are caused by currents of electricity. One of these may be typified by the cell having platinum electrodes and acidulated water for the electrolyte, in which, practically, the action is only analytical, inasmuch as water is analyzed by dissociation of its oxygen and hydrogen. Another is illustrated by the copper cell I have just mentioned. Still another, but nearly related to the first, is illustrated by the cell in which there is an insoluble anode and an electrolyte from which a metal is deposited on the cathode, while at the anode the acid of that metal's salt is set free, with perhaps a secondary reaction resulting in evolution of free oxygen thereat.

If Faraday's term, "electrolysis" is to be used to define decomposition by means of electricity, then the true electrolytic cell is the first which I have named. If it is to be used, as it has been by many, for all the cells, we need names for the three distinctive types.

As to the proper density of current to keep just below the "critical limit:"—this critical limit is a variable under conditions of kind of metal deposited, character and quantity of the metallic salt in the electrolyte, temperature and circulation of the electrolyte, etc. The more active these conditions are, the greater is the facility with which the ions are presented to the electrodes. It is as essential to success that the anions be presented to the anodes in kind and quantity as that the proper cations be presented to the cathode. Therefore, "circulating the electrolyte towards the cathode" would not have its alleged effect, if such a procedure did not likewise, as it does, cause a similar movement of the liquid toward the anode in the circulatory movement.

But this "critical limit" of density to prevent simultaneous deposition of hydrogen cannot be observed with all metals. In the art of electro-gilding, hydrogen is always evolved with the gold, and the gold is deposited in reguline condition. Tin and lead are deposited in crystalline, arborescent forms; tin with evolution of hydrogen; lead without.

There is no evolution of hydrogen in the parting of gold and silver, referred to by Dr. Sheldon, though the silver is deposited in arborescent, crystalline form. In the electrolytic assay for copper, the current density is so great, especially at the end of the deposition that the copper would be deposited as a black powder, if the free nitric acid present did not act as a depolarizer, absorbing hydrogen, thus forming nitrate of ammonia (NH_4NO_3).

There being at the command of the electrochemist the all-pervading energy, through the agency of means called electrical, what can he not accomplish? The power of analysis and synthesis is with him. He composes, decomposes and recomposes the compounds of the elements to fill economic wants, or necessities, where nature has not supplied them in sufficient quantities at his hand; or, with his magician's wand, he converts otherwise apparently useless materials into substances of greater value to mankind.

We are upon the threshold of this temple of science in which there are glories yet to be seen and understood. There are some barriers to be removed; but the nearer we approach the great hall of the temple, less and less formidable will be the obstructions.

I wish also to say a little about the following statement in Mr. Hering's paper: "Because the mechanical resistance to the flow of liquids in pipes decreases more rapidly with an increase of diameter, than the electrical resistance; or, in other words, for a given electrical resistance, the flow of liquid with the same head, increases with the diameter of the pipe."

This is not exactly understood by me—because, setting aside the friction due to the flow of liquids through pipes, the conductivity of all diameters of columns of liquid for a given length are proportional to the square of the diameters, as in other conductors, and the flows of the liquids have the same ratio.

As far as I can gather from the paper, the substitution of rubber hose, or tubes, might have been made for the lead pipes, with the elimination of the conductivity of the sections of lead pipe. If it be the case that any given liquid is a conductor only because of the electrolytic actions which take place in it, and that the electrolytic results are a true measure of the current, what more do we need to determine its conductivity or, conversely, its resistance, than to observe the length and cross-section of the electrolyte, and the difference of potential between the anode and cathode, when the surfaces of both electrodes are of like material; as, for example, copper in a cupric sulphate solution wherein analysis and synthesis are equal and opposite, and Ohm's law is directly applicable. In case of electrolytes where there is polarization, then the applied electromotive force minus the counter-electromotive force of the cell divided by C will equal R. But, so far as I can gather from the paper, the practical purpose which was to be served might as well have been satisfied by the direct application of Ohm's law, considering the counter-electromotive force, if any existed, as a simple resistance. The

method is ingenious, and I trust it may have practical application, but I think it is subject to sources of error due to the instability in the number of ions presented to the electrodes, arising from movements in the electrolyte and movements of the electrodes. At rest, a higher resistance will be shown than would be the case if the electrodes are kept at constant parallel distances apart with a transverse movement across the tank.

In considering the subject of electrolytic conduction without electrodes, I desire to write a formula or two on the board to show one way that the question whether a flow of current takes place in the case of the arrangement devised by Mr. Hering may be illustrated. Thus:

$$\left\{\begin{matrix} H_2 & O & H_2 & O & H_2 & O \\ Cu & (SO_4) & Cu & (SO_4) & Cu & (SO_4) \end{matrix}\right\}$$

$$\left\{\begin{matrix} (SO_4) & Cu & (SO_4) & Cu & (SO_4) & Cu \\ O & H_2 & O & H_2 & O & H_2 \end{matrix}\right\}$$

$$\left\{\begin{matrix} H_2 & O & H_2 & O & H_2 & O \\ Fe & (SO_4) & Fe & (SO_4) & Fe & (SO_4) \end{matrix}\right\}$$

$$\left\{\begin{matrix} (SO_4) & Fe & (SO_4) & Fe & (SO_4) & Fe \\ O & H_2 & O & H_2 & O & H_2 \end{matrix}\right\}$$

$$6\,Fe\,SO_4 + 6\,H_2O + Aq = 2\,Fe_2(SO_4)_3 + Fe_2O_2 + H_6 + 3\,H_2O + Aq$$

If we consider these ions as being the liquid in the tube or in a hose of some special form, as suggested, having sulphate of copper solution in water, it might be said that any decomposition that would take place would be counterbalanced by composition, and that it would not show that any action had taken place.

By substituting a solution of ferrous sulphate in water, having the same circuit without any anode, if there be any current induced in such a long tube, and of sufficient electromotive force, (very little electromotive force would be necessary if any current will flow at all), the result will be a combination of the ferrous sulphate with oxygen, which would be set free from the water, producing a result which may be expressed in this way: six of ferrous sulphate, plus six of water, plus an indefinite quantity of water of solution. We have, then, as a result two parts of ferric sulphate, plus ferric oxide, which would be deposited, plus six of hydrogen, which would be set free, plus three of water from the compositions and decompositions, plus the quantity of water which is present as a solvent (see equation above).

MR. W. R. WHITNEY:—I would like to answer the remarks of Prof. Doremus. I would be glad to be held responsible, if I could, for the application of the word " colloid " to what we now call " suspension," but I cannot be credited with it. It is a common term in the literature of the subject, and all suspensions

which have recently been made are called colloids by the persons who have made them and investigated their properties.

I think that future investigation will show that they are of the same nature and that all materials which we have been calling colloid, such as gelatines, and all that class of substances, are suspensions, pure and simple. We see this more particularly when clay is suspended in water, but the properties are identical, and I cannot be credited with the application of the term. It is just like the word "automobile". It is a word that we would like to get rid of, but the term "automobile" has come, and it has come to stay.

MR. EDWARD P. THOMPSON:—The longest way round is the shortest way home, perhaps, in electrical theories as well as in domestic affairs, but it seems to me that colloids and their actions may be explained on mechanical principles rather than electrical, and that we are going out of our way when we assume that colloids are supported in a liquid by some electrical force acting against gravitation.

To begin with, consider emulsions, or liquids in which minute globules of oil are suspended. Let it be an emulsion of oil and water. It seems to be going a long way around to call in electric energy for an explanation of this phenomenon. Oil is so nearly of the same specific gravity as water, that it may be said, personifying it, that it hardly knows whether to sink, float, or remain suspended. Adhesion, which need be but slight, settles the doubt, so that it is apparent that the adhesion of the molecules of the water counterbalances the tendency of the oil to rise. Passing to water containing platinum in suspension, the subdivision is so extreme that the particles approach, for all practical considerations, the size of molecules, and may, for all we know, locate themselves in the interstices among the molecules of the water, somewhat as shot would among bullets and bullets among cannon balls in true solution.

Or, it is easily imaginable that the dust of platinum adheres to the water particles with sufficient force to overcome gravitation, in the same manner that any kind of dust will remain upon a ceiling against the downward pull of gravitation. Why call in electricity for these simple cases until some experimental verification is offered—unless for purposes of discussion? It is well that Prof. Whitney has brought up this question and shown the gist of the subject as it now stands.

On the other hand, a simple imaginable or old experiment seems to me to discourage the presence of electrical discharges. Let the platinum be not in almost atomical form, but tangible, say, simply a coin. Let this be attached to the end of an insulating handle or glass rod, and the coin charged either with positive or negative electricity. No sooner does the coin touch the water than the charge is dissipated; that is, distributed throughout the large mass of water and the containing vessel. On the same principle, any charge on the platinum or other "colloid" would not remain an instant after touching the water.

Electricity does not play any part in the precipitation, as far as I can see, and purely mechanical explanations seem to be more probable and satisfactory. The addition of an electrolyte in a very small quantity throws down the colloids. The calling of a reagent by the name of electrolyte is favorable to the electrical theory. There are no electrodes present, so why call the solution of salt, or other chemical, an electrolyte? It is nothing more than a liquid which will freely mix with and diffuse itself, or dissolve, in the water or that liquid which suspends the colloids. The paper shows that some of the reagent or salt solution goes down with the colloids; therefore, this is sufficient evidence that the adhesion between the colloids and the added solution is greater than that between the original liquid and the colloids. If too much reagent is added, the precipitation is interfered with, —why? Possibly, because the particles of reagent are so numerous that they scatter themselves, together with their minute load of colloids, throughout the mass as well as at the bottom of the containing vessel.

Regarding colloids approaching and receding from electrodes, because they contain charges, why is it not more easily clear that the electrodes themselves give them the infinitesimal charge necessary and then attract or repel according to well-known principles? An electroscope is actuated by a charged electrode before the electroscope is first charged, and particles floating on water will gradually be attracted to the sides of a vessel without resorting to electrical explanations. There is a chance for crucial experiments, in which a great deal of ingenuity will have to be displayed. We may now be able to say that electricity positively plays a part in dissociation by electrodes in a liquid through which a current is passed or by which a current is generated. Experiment has proved it; but that colloids remain suspended, and are thrown down by reagents by any kind of electrical force still needs proof and confirmation.

Dr. Loeb, as I remember his paper, refers to thin films of an insulator about colloids in living animal and vegetable bodies, and the puncturing of these bags lets out the electric charge from within and produces pain. I am not ridiculing his valuable speculations which, as Prof. Whitney intimates, have set others to work and have added new facts, but we generally find electrical charges on the outside of any charged mass, and not on the inside.

My interest in this subject and my theory of a mere mechanical and not an electrical cause of the phenomenon, are due to my acquaintance with a new non-electrical method of throwing down colloids, invented by a client, but still kept secret in view of obtaining patent protection.

MR. C. J. REED:—There are quite a number of interesting points for discussion in these papers, but as there are only a few with which I want to take issue, I will do it briefly.

In Dr. Sheldon's paper I think he stated that the apparatus

described here is a condenser; that seems to me incorrect. It is not a condenser, but an accumulator. If we pass an electric current through zinc sulphate from a carbon to a zinc plate, or between two carbon plates, and deposit zinc, we are simply accumulating chemical energy which can be expended again in the form of electrical energy. Such an apparatus acts just as any other accumulator. A condenser, as I understand it, accumulates the electric charge. There is no evidence that anything of that kind would be accumulated here.

In another part of the paper there is a statement in regard to the production of lead by means of hydrogen. That statement is in line with an assumption, which has been made from time immemorial, that hydrogen in a nascent state, or a state of being born, has more energy than it has in any other state. Now, there has never been any proof of that, and I have never been able to find any experimental or other proof in any of the literature on the subject to show that nascent hydrogen differs from ordinary hydrogen, or can do anything that ordinary hydrogen will not do. I recently made some experiments to see whether nascent hydrogen would reduce lead from any of its compounds and I found that it would not. I found that nascent hydrogen would not only not reduce any lead, but would reduce no other metal which is electro-positive to hydrogen; that is, which combines with greater energy.

It is easy to show that lead by oxidation will produce nascent hydrogen, and if nascent hydrogen will reduce lead, why not set them both going at once and we shall have perpetual motion? Finely divided lead or tin in dilute hydrochloric acid and in contact with platinum will produce nascent hydrogen abundantly at the surface of the platinum; but if a compound of lead, such as the chloride or oxide, be introduced in contact with the platinum, there will be no reduction of metallic lead.

I think the mistake has always been in assuming that, because nascent hydrogen is produced by the same agent which simultaneously reduces the more electro-positive metals, therefore, the nascent hydrogen is the agent which does the reducing. As a matter of fact, it is only a coincidence. The hydrogen and the other substances are reduced by the same agent.

You are all familiar with the Smee battery in which zinc oxidizes and in which hydrogen is reduced on the platinum pole. There is plenty of zinc in the solution adjacent to the platinum or platinized silver plate and none of the zinc is reduced. It shows that the nascent hydrogen there cannot reduce the zinc. The same may be shown with every material which itself is capable of evolving hydrogen from any acid or liquid.

There is one point in Dr. Whitney's paper that I would like to call to your attention. He says:

"Thus far there seems to be evidence that the coagulation corresponds to the conception of a neutralization of the charges of the suspended particles by oppositely charged parts of the electrolyte."

By this I understand him to mean oppositely charged ions which have been dissociated electrolytically according to the theory of electrolytic dissociation. As I understand it, the positively charged ions and the negatively charged ions must necessarily be equal at all times in an electrolytically dissociated solution. If these suspended particles having a charge of one kind can be neutralized by the charges of the opposite ions in the solution and so become precipitated, then what becomes of the remaining charged ions? This would leave free ions of one polarity in the solution, but this is contrary to the very theory which attempts to make this explanation.

Referring to the paper of Mr. Hering on electrolytic conduction without electrodes, it seems to me that if it is possible by any means to get a continuous current, or an interrupted direct current, in a ring or chain of electrolytes, it would perhaps help us to settle some very important theoretical questions. It seems to me that if there is any possible way of getting at it, it is worth while working for. It ought to settle pretty definitely some questions relative to the theory of electrolytic association. It seems to me impossible that there could be any chemical change in a ring consisting of a single uniform electrolyte, through which a current passes without electrodes. Whether we assume that the substances are electrolytically dissociated or not, the ions must continue in the same proportion in all places.

But it would be possible, if we could get a current through an uninterrupted chain of electrolytes, to arrange several electrolytes in series, as suggested by Mr. Hering, so that certain ions could be made to move from one part of the chain into another, provided the electric current *per se* causes the migration of the ions. That is a part of the theory of electrolytic association of which I am not convinced at present.

I recently made some experiments which I cannot describe here now, but which seem to show that the migration of ions depends not simply upon the current, but upon the electrochemical action of the electrodes. For example, in a chain of electrolytes I have an isolated portion of copper sulphate with a different electrolyte on both sides of it. With a copper anode and a copper cathode, a certain amount of copper is transferred out of this isolated portion through a porous partition with a given current extending through a given time. That is, with a certain number of amperes, we get a certain number of grammes of copper transported electrolytically through a porous partition. If we make no change in the apparatus except to substitute other electrodes, which results in a different electrochemical action at the terminals of the electrolyte, we get only half the amount of copper transferred for a given amount of ampere-hours of current, and yet there is no change in the conditions concerning this isolated portion of copper sulphate. The same current maintained for the same time, causes a much less quantity of copper to pass out.

Three experiments of this kind were tried under different con-

ditions, and in one there was a reduction of 50%, in another of 30% and in another of about 20%. I do not care to describe these experiments until after I have had a chance to repeat them with much more elaboration.

It seems to me, however, that if we consider the energy in an electrochemical action, we must come to the conclusion that, after all, the migrations are due to chemical energy only; that the electric current merely decomposes at the terminals of the electrolyte and leaves residual unsaturated constituents of the molecules which must neutralize each other by chemical action, that is, by an interchange of ionic constituents in the molecules, and that this is what causes the migration. For instance, in my conception of the transfer of copper, in copper sulphate, it is something like this: the copper ions or atoms (I make no distinction between them), may be considered as a rod of copper extending from one electrode to the other, and the SO_4 ions, another rod of SO_4 lying parallel with the copper rod. When an atom of copper is taken off the copper rod at one electrode and an atom of SO_4 is taken off the other rod at the opposite electrode, then the two rods will still be parallel, but the ends will not be even, and the chemical affinities will tend to make them even, and that will necessarily cause a motion of the copper in the one direction and of the SO_4 in the other.

MR. HERING.—Dr. Keith suggested the use of a rubber tube. He will find it stated in the paper that rubber, as well as everything except lead, is excluded by the nature of the liquid, which was highly corrosive. Concerning his remark about the diameter of the pipe, I can only say that time is too short to go into the mathematical proof here, but he will find that what I said in the paper follows mathematically from the laws of the flow of liquids in pipes and those of the electrical resistances. The laws concerning the relation of the diameter to flow of liquids in pipes, are not the same as those for the flow of electricity through solid conductors, and it is due to this difference that the relation exists which is given in the paper.

Concerning his suggested method of determining the conductivity of liquids, I think he would find if he tried it, that he would get probable or unknown errors of very large amounts. As for getting anything approaching accuracy, I think he would find it quite impossible.

There are quite a number of things in the papers of this evening which I would like to discuss, but unfortunately the time will not permit. I would like especially to discuss much of what Mr. Reed has just said, as I differ very decidedly with him in many points, but I will limit myself to the papers.

Dr. Sheldon says something about electrolytic condensers. I agree with what Mr. Reed said, that this is nothing more than an accumulator, a one-legged accumulator in this case, because one plate is dead. The action of the true electrolytic condenser, as I understand it, is something quite different from what Dr.

Sheldon describes it to be, but the hour is so late now that I cannot go into that.

Mr. Jacobs, I think, uses the words " heat " and " thermic " in a somewhat misleading sense, although the same is done by many other authors. The heat of formation, so-called, is not really heat at all; at least, not necessarily so; it is energy, which may be converted into several other forms of energy, only one of which is heat.

He shows how hydrogen may be produced cheaply and in very large quantities, but the question naturally arises, what is to be done with all of that hydrogen? Is there a market for such quantities?

Referring to Dr. Keith's paper, I would like to ask how he gets out the gold and silver? It seems to me that one of the important features in a copper reduction process is the recovery of the gold and silver which are associated with the copper, but in his method the gold and silver will go down as sludge with the large mass of the powdered rock, and therefore make a very poor gold and silver ore.

DR. KEITH:—I will say that in this particular case which I considered, gold and silver were not present in sufficient quantity to be of any commercial value.

PRESIDENT STEINMETZ:—I may say regarding the name electrolytic condenser, that the name has been introduced and has become standard in electrical engineering terminology, for the reason that a polarization cell, as, for instance, two platinum electrodes in diluted acid, when exposed to an electric potential behave analogously to an electrostatic condenser. A polarization cell, when exposed to alternating electromotive force, below the voltage of dissociation, acts like a condenser in so far as the alternating current in the cell leads the electromotive force ninety degrees or less, (by the energy lost in the cell), and, therefore, the polarization cell has been proposed very largely as a substitute for the electrostatic condenser and is used to a limited extent.

Regarding the colloid question and the discharge of the suspended particles, this is not necessarily the case. If you say the suspended particles in the colloid solution have electrostatic charges, or as you may perhaps better express it, are at an electric potential different from the electric potential of the solution, it is not necessary that even in an indefinite time the potentials should be equalized. There is one characteristic difference in the electric phenomena from some other phenomena, as for instance, the phenomena of heat. The potential function of heat must be continuous; there can be no sudden and abrupt change of potential, or temperature, at the boundary line of two substances, but the temperature must always be a steady function of space. It is different, however, with the electric potential function. We know that in a state of equilibrium there is an abrupt change, or discontinuity in the electric potential function

at the boundary line of two substances. For instance, if you have a copper and a zinc plate in water or any other solution, which does not act chemically thereon, then no chemical action will take place, and there will be an equilibrium retained forever, but there will be a difference of electric potential between the zinc and the copper. In the metal, the potential function is continuous, and in the conducting liquid between the two, it is continuous also. It follows, therefrom, that there is a discontinuity of electric potential at the boundary line between the metals in the solution. Hence, there may be in the state of equilibrium a discontinuity in the electric potential function between the suspended colloid and the liquid in which it is suspended, and this discontinuity, this difference of potential, exists and is necessary for the state of equilibrium, while any electric stress disturbing this discontinuity, or difference of potential, will cause a disturbance of the equilibrium and thereby precipitation of the colloid. For instance, introducing an electrolyte with ions which have a different discontinuity of the potential function, the difference between such ions and the suspended particles of the colloid will cause a disturbance of the potential function which may cause precipitation. So you will see it is very possible with electric potential functions to have a discontinuity or constant difference of potential existing in a state of equilibrium at the boundary line of two substances, while with other forms of energy, as heat energy, for instance, such discontinuity cannot exist.

If there is no further discussion I shall call upon the authors of the papers to conclude.

MR. WHITNEY:—In reply to Mr. Reed's question as to where the negative ion of the coagulating salt goes: this occurred to us in the experiments. If we assume negative colloids to be precipitated or coagulated by the positive ions of the salt, say barium, of barium chloride, the negative charge of the chlorine is to be accounted for. The negative charge of the chlorine must go somewhere. When the experiment was tried, hydrochloric acid was found free in the liquid. We found the barium carried down with the colloid and free hydrochloric acid in the solution. It seems to me that this in a sense explains away that difficulty. This means that the colloids act as hydrolyzing agents. The salt is decomposed, giving the hydrate which stays with the colloid and the acid which remains in the supernatant liquid.

MR. REED:—I do not see how this accounts for the precipitation of the suspended electrostatically charged particles, as it would leave free ions of one polarity in solution.

DR. KEITH:—I would like to say something on the matter of colloids. In the electrolysis of sulphate of copper solutions, using carbon electrodes, I have found that the solution was very much darkened, and that this dark solution would filter; but carbon was deposited with the copper. If carbon existed in this solution in a colloidal condition, as has been stated by Prof.

Whitney, then this colloid must have followed one of the ions, that is copper. in its deposition; or else the solution was one of carbon which was an electrolyte, and carbon was deposited by electrolytic decomposition.

DR. SHELDON:—Concerning Dr. Keith's statement relative to the increase of the critical current density, that the electrolyte must be also circulated toward the anode, I have tried a number of experiments on anodes of various sizes, and I find that the changes have no influence whatever on the critical current density of deposition.

Concerning the production of nascent hydrogen in the production of a sponge lead, whether the hydrogen is nascent or not, I do not know, but that litharge, placed on those lead electrodes, is converted into sponge lead, I do know to be a fact.

In regard to the use of the term electrolytic condenser for the cell which I have described, I need to make no apology, seeing that I have such an eminent authority as President Steinmetz on my side.

I am delighted to think that the writing of this paper has brought out such an explanation from the President of the Ampere Electro-Chemical Company, concerning the subject of the manufacture of camphor and artificial rubber.

MR. JACOBS:—In reference to the criticism as to my use of the term thermic conditions, what I said was that when electrolysis did take place in the electric furnace, even then the effects depended solely upon thermic conditions—what I meant was this, that in order to do work—to break up or decompose a chemical compound—you have to move against a counter force of a definite amount of energy, which is represented by the heat of formation of the compound in question. Knowing this heat of formation, the voltage required may be calculated and from that data and the electrochemical equivalent of the compound, you may find your working conditions, and determine the electrical energy necessary. In that way I mean that the results are dependent on thermic conditions, meaning energy—the energy of the chemical combination. In reference to the sale of pure hydrogen, I will not make any comment. I think uses will be found for it.

On motion the meeting then adjourned.

[COMMUNICATED AFTER ADJOURNMENT BY MR. C. J. REED.]

In the paper of Mr. Jacobs the statement regarding the energy required for the "fixation" or oxidation of nitrogen is likely to give an exaggerated impression as to the amount of energy necessary for this purpose. The amount is really insignificant and none at all is absorbed endothermically, except when the reaction takes place at a temperature above the boiling point of N_2O_5.

The formation heats of the oxides of nitrogen were investigated and published by Berthelot in 1875 (Annales de Chimie et de

Physique [5], VI., 151; 1875). He found the following formation heats:

N_2O_5,	gaseous	− 1.2	kilogram calories.
	liquid	+3.6	"
	crystallized	+11.9	"
	dissolved	+28.6	"
N_2O	gaseous	− 20.6	"
	liquid	− 18.8	"
	dissolved	− 14.4	"
NO	gaseous	− 21.6	"
NO_2	gaseous	− 1.7	"

For the formation of N_2O_5, therefore, in the solid, liquid, or dissolved condition, no heat is required, but the reaction or combustion evolves heat. For the formation of gaseous N_2O_5, as it would be formed, for instance, in the electric furnace, there is required only 1,200 gram-calories for a gram-molecule or 108 grams of N_2O_5; that is, 11.1 gram-calories per gram of R_2O_5. This would be equivalent to the quantity of heat required to raise the temperature of a gram of water 11.1°, and is really insignificant from a commercial standpoint.

The practical difficulty in the way of direct combustion of nitrogen to N_2O_5, is due to the very high temperature required for the ignition and the heat required to bring the reagents to this temperature. The difficulty and inefficiency of the process is further greatly increased by the fact that the atmospheric proportions of oxygen and nitrogen are very different from the proportion in which these elements combine to form N_2O_5, and that, therefore, a large quantity of unconsumable nitrogen must be raised to this high temperature and then rejected.

The proportions by weight of oxygen and nitrogen in the atmosphere are 23 of oxygen to 77 of nitrogen, while the combining proportions are 80 of oxygen to 28 of nitrogen. For every gram of nitrogen consumed, therefore, 9.6 grams must be raised to the temperature of ignition. The result is virtually the same as if the specific heat of nitrogen were multiplied by 9.6. This indicates that the successful commercial solution of the problem would be in supplying oxygen in the proper proportion, or else in bringing about the combination indirectly by oxidizing the nitrogen at a low temperature.

Another possible solution would be in producing first a lower oxide and subsequently further oxidizing it. Of the other oxides the most favorable for the production of HNO_3 would be NO or NO_2. But even in these the proportions of nitrogen and oxygen are very different from the atmospheric proportions. The endothermic absorption of 21.6 kilogram-calories in the formation of NO is of no great practical importance, as this heat is evolved again in the second process of further oxidation.

From the facts stated, it is apparent that the inefficiency of

artificial processes for the fixation of nitrogen is not due to the absorption of energy in the chemical reaction, so much as it is due to the final rejection of energy in the form of unavailable heat, which escapes in the unused nitrogen.

[COMMUNICATED AFTER ADJOURNMENT BY MR. E. P. THOMPSON.]

Having opposed, in my discussion at the meeting, the electric theory regarding colloids or particles suspended in a liquid, it is incumbent that I should present a better theory if possible. I think I can dispose of all theories, and clearly prove the principle upon which this phenomenon rests. The premises are, in the concrete, that a cubic foot or cubic inch, or other comparatively large piece of platinum, will sink rapidly in water, while a piece approaching the size of a molecule—that is, a very small piece or particle, or a colloid, as termed in the Whitney paper, apparently does not sink, but remains suspended in the water indefinitely. I have performed experiments, and I find that colloids are continually sinking, provided the liquid is maintained as free as possible from circulation by changes of temperature, and free from jars of passing wagons, walking on the floor, and so forth. Any stirring or other motions, however slight, as by slowly heating the liquid, will cause the colloids to be wafted as if on wings, into the mass of the liquid. My problem to be proved is, why does a large piece of platinum sink fast and a small particle very very slowly? I decided that it was due to a less reduction of surface in proportion to reduction of mass. If people were as small as colloids, they would have no trouble in flying, because their weight or mass would be almost zero, while the surface would be infinity almost, in comparison. Birds are small animals. Even an eagle is small compared with beasts. The same is true of fish—allowing for a few exceptions. This conception and explanation assist in understanding the problem of the flying of birds.

It may be said that a molecule or atom is practically all surface. Let this be proved first by a specific converse example. Imagine a cube of platinum. It is, say, a cubic foot, There is a square foot of surface on each side, and, therefore, there are six square feet of surface. Now place another cube on top, so as to make a mass of two cubic feet. Now, how many square feet are there? Twice as many? No, only ten square feet. If in a minute particle the radius is 1/1,000 of an inch, the surface has 300 times as many square inches as the mass has cubic inches. Similar calculations will show how minute is the mass in acolloid in proportion to its surface. Consequently, it falls through the liquid on the principle of a parachute or a feather in air, or a horizontal sheet of paper in water, except that the speed is proportionally less.

The same principle explains the floating of smoke, which usually falls several feet from where it issues even in quiet air; but I can call attention to a smoke that falls twelve miles distant.

It may be seen from New York Harbor, at Bayonne, N. J., a faint, white smoke drifting in the direction of a breeze. As far away as Westfield, N. J., twelve miles, it may be detected by the nose. It is from an oil refinery. With a Southern breeze, it is recognizable in any part of New York City. The particles of smoke are colloids in air, and the air may be ever so quiet and yet the white particles appear to be held in suspension like other colloids. It may be concluded, I think, that colloids are not truly held in *permanent* suspension, but are in the process of *sinking* in an absolutely quiet liquid, and that they do not sink fast, because the mass or weight of a particle diminishes in much larger proportion than its surface, and I conclude further, that, whether any electricity is present or not, and independently of the curious action of electrolytes upon colloids—that colloids sink very slowly in a liquid free from circulation, because the surface of a colloid is enormous in proportion to its weight.

[COMMUNICATED AFTER ADJOURNMENT BY MR. C. F. BURGESS.]

In regard to the paper by Mr. Carl Hering on "Electrolytic Conduction without Electrodes," I might add still another way for setting up and detecting the flow of current in an electrolytic conductor without the use of electrodes. Mr. A. B. Marvin, of the department of Applied Electrochemistry of the University of Wisconsin, has devised and tested a number of methods for detecting the flow of current induced in an electrolyte, a number of which were mentioned in Mr. Hering's paper, and from the results of such tests it seems very conclusive that the flow of current without electrodes is very similar in what are known as electrolytic conductors and in metallic conductors. The difficulty in demonstrating this fact lies in the enormous difference existing between the conductivities of the two classes of conductors. One of the methods which apparently bears the stamp of novelty was the introduction of a good conducting electrolyte in place of the squirrel-cage armature of a three-phase induction motor. It was thought that if a rotation either of electrolyte or the vessel in which the electrolyte was contained could be produced, it would be a demonstration of the fact that currents were set up in the electrolyte without electrodes. This was found to be the case. In rotating the field in one direction, the electrolyte began to revolve after a short time, and on reversing the direction of the rotating field, the electrolyte came to a rest and then slowly reversed its direction.

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

NEW YORK, March 28th, 1902.

The 163rd meeting of the INSTITUTE was held at 12 West 31st Street, this evening, President Charles P. Steinmetz in the Chair.

PRESIDENT STEINMETZ:—Gentlemen, the meeting will be in order. I give the floor to Secretary Pope, to make some announcements.

SECRETARY POPE:—At the meeting of the Board of Directors this afternoon the following associates were elected:

NAME.	ADDRESS.	ENDORSED BY
BEVENUE-MILLER, EDWIN DAVID,	Electrician, Natal, Government Railways, Essex Road, off Umbilo Road, Durban, Natal, So. Africa.	W. D. Weaver. R. W. Pope. Edward Caldwell.
BROWN, HUGH AUCHINCLOSS,	General Utility Engineer, Crocker-Wheeler Co., Ampere, N. J.; residence, 285 Springfield Ave., East Orange, N. J.	Horace Dowie. S. S. Wheeler. Gano S. Dunn.
BULLEN, DANA RIPLEY,	Salesman, General Electric Co., 200 Summer St., Boston; residence, Winthrop, Mass.	Louis Bell. Harry Bottomley. S. B. Paine.
BURTON, FRANK VAIL,	Office Salesman, Western Electric Co., 463 West St.; residence, 475 Central Park West, New York City.	G. F. Atwood. G. A. Hamilton. P. G. Burton.
CLARK, EUGENE BRADLEY,	Chief Electrician, So. Works, Illinois Steel Co.; residence, 5342 Cornell Ave., Chicago, Ill.	C. F. Scott. H. J. Ryan. R. D. Mershon.
COSBY, FRANK CLARK,	Salesman, Westinghouse Elec. and Mfg. Co., 120 Broadway; residence, 124 West 114th St., New York City.	C. F. Scott. H. J. Ryan. H. W. Fisher.
DAVIDSON, JAMES EDWARD,	Superintendent, Port Huron Light and Power Co., Port Huron, Mich.	R. W. Van Vleet. Alex Dow. H. G. Field.

DICK, WILLIAM AMZI, Designing Electrical Engineer, Westinghouse E. and M. Co., Pittsburg, Pa. — N. W. Storer. C. E. Skinner. C. F. Scott.

FANSLER, PERCIVAL ELLIOTT, Post-Graduate Student, Purdue University, Lafayette, Ind. — W. E. Goldsborough. C. P. Matthews. J. W. Esterline.

FISH, FREDERICK PERRY, President, American Telephone and Telegraph Co., 125 Milk St., Boston; residence, Brookline, Mass. — Elihu Thomson. C. S. Bradley. H. V. Hayes.

FOLK, FREDERICK JACKSON, Chief Electrician, New York and Queens E. L. and P. Co.; residence, 135 Rutland Rd., Brooklyn, N. Y. — W. B. Jackson. Harris J. Ryan. H. A. Lardner.

GRANT, OLIVER REMICK, Student, Columbia University; residence, 2236 Southern Boulevard, Bronx Borough, New York City. — F. B. Crocker. G. F. Sever. F. Townsend.

GRISSINGER, ELWOOD, Engineer, The Cataract Power and Conduit Co., 40 Court St., Buffalo, N. Y. — H. W. Buck. P. M. Lincoln. C. F. Scott.

HALL, CLARENCE MORTIMER, Teacher of Physics and Electricity, Manual Training School No. 1, 17th and R. I. Ave., N. W., Washington, D. C. — H. B. Smith. L. D. Bliss. F. R. Jones.

HODGE, CHARLES, Salesman, Westinghouse E. and M. Co., 120 Broadway; residence, 72 E. 34th St., New York City. — C. F. Scott. R. D. Mershon. P. T. Brady.

HOGAN, CHARLES WILLIAM, Photometric Expert, Columbia Incandescent Lamp Co.; residence, 3028 Bell Ave., St. Louis, Mo. — J. E. Randall. H. B. Shaw. R. W. Pope.

JOHNSON, WOOLSEY MCALPINE, Electro-Metallurgist, Oxford Copper Co., New Brighton, S. I., N. Y. — Wm. Stanley. W. L. Robb. W. D. Weaver.

KNIGHT, PERCY HENRY, District Engineer, Westinghouse E. and M. Co., San Francisco, Cal. — C. F. Scott. N. W. Storer. C. E. Skinner.

MILLER, ALPHONSUS JOSEPH, Laboratory Assistant, Western Electric Co., 463 West St.; New York City; residence, 161 Newark St., Hoboken, N. J. — G. A. Hamilton. D. E. Bicknell. Norman Leeds.

MURPHY, EDWIN J., Assistant Engineer, General Electric Co., Lynn. Mass. — W. C. Fish. F. P. Cox. E. E. Boyer.

MCALLISTER, ADDAMS STRATTON, Assistant in Physics, Cornell University; residence, 106 Catharine St., Ithaca, N. Y. — H. J. Ryan. E. L. Nichols. J. P. Jackson.

MCGRATH, WILLIAM HENRY, Electrical Engineer, with Stone & Webster, 930 Federal St., Boston; residence, 27 Pleasant St., Quincy, Mass. — C. A. Adams. S. E. Whiting. Russell Robb.

PHILLIPS, ELLIS LAURIMORE,	Engineer, Westinghouse, Church, Kerr & Co., 8 Bridge St.; residence, 11 West 103d St., New York City.	H. J. Ryan. J. J. Bellman. P. H. Powell.
PIERCE, ALFRED LAWRENCE,	Superintendent, General Manager and E. E., Borough Elec. Works; residence, 102 N. Whittlesey Ave., Wallingford, Conn.	A. E. Winchester. E. C. Boynton. J. B. Cahoon.
PIRTLE, CLAIBORNE,	Sales Agent for North Carolina General Electric Co., Box 301, Greensboro, N. C.	E. J. Berg. W. L. R. Emmet W. I. Slichter.
RADTKE, ALBERT AUGUSTUS,	Instructor of E. E. and Physics, R. I. College of Agriculture and Mechanic Arts, Kingston, R. I.	D. C. Jackson. B. V. Swenson. C. F. Burgess.
REYNOLDS, LOUIS EMBREE,	Chief Electrician, Independent Light and Power Co., 321 Market Street; residence, 460 Noe St., San Francisco, Cal.	W. H. Allen. G. P. Low. W. R. Hewitt.
RUGG, WALTER S.,	Engineer, Westinghouse Elec. and Mfg. Co., 120 Broadway; residence, 225 W. 83d St., New York City.	L. B. Stillwell. H. J. Ryan. B. J. Arnold.
SCRIBNER, CHARLES E.,	Engineer, Western Electric Co., 259 South Clinton St., Chicago, Ill.	J. J. Carty. B. Gherardi, Jr. J. C. Rennard.
SESSIONS, EDSON OLIVER,	Construction Engineer, General Electric Co., Toledo, Ohio; residence, Schenectady, N. Y.	C. P. Steinmetz. F. O. Blackwell. T. C. Martin.
SHERWOOD, EDGAR F.,	Superintendent of Traffic, New York Telephone Co., 18th St. and Irving Place, New York City; residence, 59 Park Ave., East Orange, N. J.	J. J. Carty. B. Gherardi, Jr. J. C. Rennard.
SMITH, WILLIAM NELSON,	Electrical Engineer, Westinghouse, Church, Kerr & Co., 8 Bridge St., New York City; residence, 62 Morris St., Yonkers, N. Y.	G. L. Hoxie. Carl Hering. Edward Caldwell.
SPENCER, HARRY B.,	Clerk in Draughting Department, General Electric Co., Lynn, Mass.; residence, Proctor, Vermont.	Elihu Thomson. N. S. Hopkins. E. E. Boyer.
WHITE, LINDEN G.,	Superintendent Electrical Department, Columbus Edison Electric Light Co., Columbus, Ohio.	C. D. Haskins. F. C. Caldwell. W. S. Barstow.

Total, 34.

At the same meeting the Board of Directors made the following nominations for the coming election:

For President—Charles F. Scott; *for Vice-Presidents*—C. O. Mailloux, Bion J. Arnold, Schuyler S. Wheeler; *for Managers*—Townsend Wolcott, Gano S. Dunn, E. H. Mullin, Charles A.

Terry; *for Treasurer*—George A. Hamilton; *for Secretary*—Ralph W. Pope.

PRESIDENT STEINMETZ:—Gentlemen, Members of the INSTITUTE and Guests: The topic of to-night's discussion is power transmission, but power transmission different from that which we have considered in previous meetings. It is telephony. It may be of interest, then, briefly to compare telephonic communication with long-distance transmission of large power, to see the similarity and the difference between them.

In telephony, as well as in long-distance transmission of large power, alternating currents are used exclusively. While, however, in long-distance transmission large powers of thousands of kilowatts pass through the circuit, the power of the telephonic transmission is exceedingly small, since the potential energy available in a pound of coal is sufficient to give an audible sound in a telephone receiver for millions of years, I do not remember how many.

In long-distance power transmission, from one or from a number of generating stations, power is transmitted through the circuit to one or a number of receiving stations. Thus, the stations are either generating or receiving stations, and the power flows over the circuit more or less continuously in one and the same direction. It is different, however, in telephonic communication. Here each one of a large number of stations is a generating station, as well as a receiving station in rapid succession—a generating station when speaking, a receiving station when listening. The problem here is to be able to transmit from any one of a large number of stations, power to, and receive power from, any one of a large number of stations. Hence, it is essential that the stations should be normally disconnected from each other and be able to be connected together in any desired manner, and here appears the necessity of the central station and the switchboard. I do not need to dwell upon this any further, since it will be taken up in the papers that are to be presented.

The most interesting characteristic feature and difference between telephonic transmission of speech and long-distance transmission of power, next to the difference in the magnitude of the power transmitted, is in the purpose of the transmission and thereby the character of the transmitted power. The problem of long-distance power transmission is to deliver as large a percentage as possible of the electrical power produced in the generating station at the receiving station; that is, to reach as high efficiency as possible; but the character of the power delivered is relatively immaterial, and therefore the simplest form is usually preferred, that is, a sine wave of current and electromotive force. The character of the wave shape, its distortion, is of more or less secondary importance, and frequently of no importance at all. Entirely different, however, is the problem in telephonic transmission of speech. The sound waves are very complex harmonic waves consisting of a fundamental and many higher har-

monics or overtones, which by their frequency and their relative intensity give the character and distinctive difference between the various sounds, as the vowels and consonants, and thereby articulation and intelligibility of speech. It is the overtones which give the difference between the notes of the different musical instruments, etc. In telephonic transmission the sound waves are transformed in the generator, the microphone sender, into waves of alternating current, which are re-transformed into sound waves in the receiving telephone. As you see, the problem of telephonic power transmission, then, consists not in delivering a large percentage of the generated power at the receiving end, but in delivering at the receiving end power as nearly as possible of the same character, that is, of the same wave shape, as produced at the generating station. The problem, therefore, is the maintenance of the wave shape of the higher harmonics of the generated wave and not the efficiency of transmission. The efficiency of telephonic transmission is necessarily extremely small. Of the power supplied from the battery to the microphone circuit, probably an insignificant part of 1% is delivered as mechanical power by the diaphragm of the receiving telephone, but this is of no moment since the absolute value of the power under consideration is very small.

The capacity, etc., of the telephonic line tends to modify the wave shape and to suppress the higher harmonics; to discriminate against the higher harmonics which give the distinction between the different sounds, that is, the articulation and intelligibility of speech. Therefore, with the increasing length of the telephone line, gradually the higher harmonics decrease, articulation becomes at first impaired and then destroyed, and speech becomes unintelligible, not because power does not reach the receiving telephone in sufficient quantity to give a sound, but because the power reaches it at a wave shape different from that generated at the sending end, with the higher harmonics more or less suppressed. While in a long-distance power transmission, by increasing the impressed voltage, the transmission may always be improved, or made possible (within certain limits), no increase of power supply can make an inoperative telephone circuit operative, since the relative proportion of the components of the arriving wave does not vary with the power (except in so far as the operation of the sending apparatus may be affected by the power supplied thereto). Here, then, is a distinctive difference between telephonic and power transmission. This condition is made worse by the character of the telephone generator. By varying the resistance of the microphone contact in the circuit of the battery current, the current is made pulsating. Its pulsating component is transformed into a higher voltage alternating current, which passes into the telephone line. This telephonic generator tends more or less completely toward constant current regulation. The capacity of the telephone line absorbs a part of the current, proportional to electromotive force and

frequency, that is, a larger part of the current of higher than of lower frequency, leaving, therefore, for reception at the receiving telephone, a lesser percentage of the generated current, the higher the frequency of the current is. This means the higher the capacity of the line, the less current of higher frequency reaches the receiving telephone, until speech becomes impossible. It is this discriminating character of the telephone line which causes the difficulty of long-distance telephony.

At the same time, this explains the characteristic feature of self-induction in the telephone circuit, and the difference between its action in telephony and in long-distance power transmission. Self-induction induces a voltage proportional to the current and the frequency. While capacity is discriminating against higher frequency, self-induction discriminates in favor of high frequencies, in constant current circuits, its electromotive force being proportional to the frequency. Therefore, in a constant current circuit the self-induction requires or causes to be generated an electromotive force proportional to the frequency of the harmonics. That means, in other words, in a telephonic circuit supplied by approximately constant current self-induction intensifies the higher harmonics generated by the sending apparatus, in proportion to the frequency, and thereby reduces the result of the diminishing effect of the line capacity on the higher harmonics, and the impairment of articulation produced thereby.

Heretofore all practical work in extending the distance of telephonic transmission has been done in trying to reduce the inherent discriminating effect of the line against higher harmonics or higher frequencies by reducing the line capacity as far as possible, so as to absorb the least possible amount of current, and by reducing the line resistance as far as possible so that as large a percentage as possible of the generated current may reach the receiving telephone. Probably the practical limit of long-distance transmission has been reached, or, at least, more or less closely approached; and further improvements should, therefore, be looked for only by inserting apparatus discriminating in favor of higher harmonics, as self-induction, etc. With distributed line capacity, self-induction, to discriminate in favor of higher frequencies and thereby reduce the objectionable effect of the line capacities, will have to be distributed also.

I do not need to dwell upon this matter any further, since it has been discussed in previous papers recorded in our TRANSACTIONS. However, the limit has by no means been reached and a very great amount of work will still have to be done, and may be done in this direction of inserting appliances to discriminate in favor of higher frequencies, thereby reducing the wave shape destruction due to the line capacity, and improving articulation and intelligibility of speech, that is, extending the distance of successful telephone transmission. Practically nothing has been done yet to make sending and receiving apparatus selective and

discriminating in favor of higher frequencies, since the preliminary work done in this direction in the early days of telephony, by such investigators as Eickemeyer and others.

That brings me to the last point of which I desire to speak, and that is the telephonic relay. Thus far no success has been had with telephone relays. At the same time, it appears to me that the question of relaying telephonic currents has mostly been approached with a misunderstanding of the problem. It is not the problem to insert power into the telephone system to increase the power of the telephonic current, because we can get any power we want by increasing the voltage; the problem is to avoid the destruction of the wave shape. The telephonic relay, therefore, should insert power in such a manner as to be proportionate to the frequency, that is, discriminating in its power supply in favor of high frequency, otherwise no advantage can result over direct telephonic transmission in articulation and intelligibility. Nothing, as far as I know, has been done in this direction, although it appears to me that the problem is by no means hopeless, and I can see a number of directions in which it might be approached, although the difficulties are formidable, owing to the high frequency of the currents under consideration.

I desired to dwell upon the engineering feature of telephonic communication somewhat more fully, since the papers scheduled for to-night do not consider the subject from this point of view. I now call upon Mr. Arthur Vaughn Abbott for the first paper.

Mr. Abbott read his paper. [See p. 365, April issue.]

MR. CARTY:—Mr. President and Gentlemen: I think the INSTITUTE is to be congratulated on having such an efficient Committee on Papers, because for the first time in the history of the INSTITUTE we have had a telephone meeting. I am glad to see, even at this very late day, that the INSTITUTE evinces an interest in telephony and recognizes in this formal way that there is such a branch of electrical engineering as telephone engineering, and I think we are fortunate in having to-night two papers from such distinguished telephone engineers as Mr. Abbott and Mr. Dommerque.

I remember what a hard time the telephone companies have had in interesting the public in the fact that there is something to the telephone besides that which subscribers see on the wall in their offices and something besides that through which they sometimes hear in the instruments in their offices. Mr. Abbott very well brings out the fact that the telephone engineering features are brought to the mind of the subscriber principally from viewing an instrument, costing an insignificant sum, attached to the wall, and that subscriber, even though he may be an engineer, is apt to get the idea that that is the beginning and the end of telephony.

It being my habit each year to add to my staff some graduates from the various technical schools, my talk with them reveals the fact that it is often the case that when they have grasped the

principles of the Bell receiver and know the effect of variations of pressure upon certain conductors of high resistance, their idea seems to be that they have mastered those questions which relate to telephony; that a few weeks and one or two lectures on that subject are enough, and then all of their time is devoted to those engineering questions which involve large units, horse power, and so on; and I am glad to find that attention is called to the fact that there is a branch of electrical engineering which relates from the telephone. These two papers to-night have touched upon that subject.

I feel greatly indebted to the Committee on Papers also for the opportunity I have had of listening to the excellent remarks which our President has made on the subject of the analogy between long-distance transmission of power and the transmission of speech, and, what is more important, the difference that he pointed out between the two. I think I could dissent from the first of what he had to say. I would suggest that possibly the statement that the telephone generator, that is, the transmitter, is a constant current instrument, might possibly be made a little more accurate. I say this, subject to correction later, having in mind that the telephone circuit is never a constant current circuit, that we must always take the capacity into consideration, and that as we add to the capacity we increase the current output to the generator. That factor if neglected might lead to discordant results sometimes.

Coming to the papers proper, I think the author of the first paper, in paying a high tribute to the mathematical attainments of the President, has understated the case; but I think that he was governed somewhat by undue modesty when he went further and deprecated the fact that the telephone companies had not had the services of mathematical scholars of a high degree of ability, because we should all remember that Mr. Abbott himself, with whose mathematical attainments we are all familiar, was for a number of years in the employ of a licensee of the Bell Telephone Company, and I am sure that during that time that company had the benefit of his best efforts.

I think that this is not the time, neither is it the place, to discuss the propriety of the business policy of any of our electrical concerns. I will not undertake to dwell upon that phase of the subject, but when there is brought up here the question of the liberty of speech, I think it best to say a word. It is interesting to find, if the author of the paper is to be followed in this matter, that the developments of the electric light, the electric railway, etc., which have taken place in this country, have been in consequence of our freedom of speech. I do not assent to that doctrine; neither do I dissent from it. That is a pretty philosophical question. It might well form the theme for discussion at a meeting of the Metaphysical Society; but I think that a fact or two at a time like this is not out of place, and I will present one touching the matter of the freedom of speech. I have no

doubt that the author of the paper has been told that good telephonists should be seen and not heard. That may have been the experience of one man, but "One swallow does not make a summer," and we cannot reason from one particular case to general conclusions. So far as I am personally concerned, I have always enjoyed complete freedom of speech regarding telephone matters as well as all other matters. I have at times asked advice of my superiors as to whether certain publications would be advisable, and I have always received an answer to this effect: "You are the best judge, you know the circumstances of the case, and it rests with you to say how much you shall publish and how much you shall not publish." It is always an interesting question of ethics—it is an interesting one for the engineer particularly—to determine to what extent the data which engineers have gathered following the work of their employers, are the property of the engineers.

I think Mr. Abbott has done a very great service in calling attention to the fact that there is such a thing in telephone work as maintenance. As I said before, in a consideration such as this, a few facts are always of interest, and, in the best of good feeling and without in any way wishing to antagonize the author of the paper, but rather to bring out in a friendly way points for discussion, I call attention to one statement which I think requires a few words.

"The manufacture of apparatus, particularly of transmitters and receivers, was sedulously guarded, and the magneto bell, the receiver and solid-back transmitter stand to-day, after one and two decades of experience, unchanged and unimproved."

First, granting that this is true, does that indicate a lack of efficiency? Certainly not. Are we to judge of the efficiency of engineers by the size of their junk heap, or rather should they not be congratulated when out of a world of junk they can produce at least three pieces of apparatus which can persist for a number of years. I think there is not much doubt on that point. But, unfortunately, telephone engineers are not allowed to claim that credit. Whether for good or for ill, the facts are contrary to those stated in the paper. Let us consider the case in New York. Take first the case of the magneto bell, with which I am particularly familiar. Prior to 1889 the history of the magneto bell was the history of the junk shop, one group of bells following the other in quick succession into the waste pile. Was there to be no end of this? It fell to my lot to design a bell which I described with some publicity at a telephone meeting. Prior to that time no telephone company had used such a bell, After that meeting all of the telephone men went home and began discarding the bells of that date and putting in one which I described, and it has since become known as the "bridging" bell and has attained some prominence. Incidentally I will state that that prominence has been brought about by a controversy over a metaphysical question; the question being this: Was the

thing which Mr. Carty did an invention, or was it engineering—the question as to whether I did it not being in dispute. That bell, whether it was an invention, or whether it was engineering, went into general use. Is it any less to one's credit that it has persisted and been kept in use ever since? I think not. But, unfortunately, I am not allowed to take that position, because after the announcement of the—I was going to say invention—production of that bell, all of the magneto bells in the City of New York and in all the larger cities were discarded and this new type was installed. There were large additions to the junk heap then. Those bells continued in use up to four years ago; then, owing to progress in telephone matters, it was decided by my company to discard all those bells and to replace them by others using the bridging principle, but not the magneto, because they were to be used in connection with the common battery system.

That is such a striking contradiction, I may say, of the language of the paper that it would be grossly misleading if I did not call attention to it. Four years ago there were 30,000 or 40,000 of those bells in use in New York, all of which have been discarded and replaced since. So that as far as the magneto bells are concerned, the type has persisted, but the use of those particular bells has ceased.

Now, respecting the telephone receiver, that has been regarded as one instrument which was well-designed and which answered its purpose admirably. I think no mistake was made in so regarding it. Therefore, instead of it being an example of lack of progress, it is an example of an excellent design which should be commended. But even in that respect we are not allowed to take too much satisfaction, because in the last five or six years we have taken out all the receivers we had in use and sent them to the junk heap. That does not look like stagnation. But that is not enough. Of the new crop there has been type after type, one following on top of the other, at the cost of enormous sums of money. So that when we come to examine even the question of the receiver we find that although the "manufacture was sedulously guarded" it is not the same that we have used for the last one or two decades.

Now we come to another feature, the solid-back transmitter. A number of years ago the British Government instituted a series of tests of all the transmitters in the world, and the result was that the solid-back, or American type, was not adopted. I was greatly surprised and I spoke to Sir William Preece on the subject. I found that in making the tests he had not been advised that a special induction coil was needed to fit the solid-back transmitter, and that he was not advised that a certain amount of current, which could only be practically obtained from the Fuller battery or some equivalent type, was needed for its operation, and that he made the test with gravity batteries, giving a low volume of current and with a type of coil not suited to the

transmitter, so that his tests were rendered less valuable on that account. Other tests were made—one, I think, by the Swiss Government—the result of which was to choose the American type of solid-back transmitter. The fact is to-day that notwithstanding all the work that has been done in England by the English Government, in France by the French Government and by others, the transmitter here referred to has been regarded as the best or equal to the best. But even good as it was, that type has not persisted because within the last four years right here in New York we have removed 50,000 telephone transmitters of that type and replaced them by a modified type. So that in these three instances, which apparently would indicate that progress had ceased, we find that the facts show that improvement has gone on as before.

There is one more point about which I would like to speak, and that is the analogy between talking over the telephone and "buggy riding." It is said that when we hire a rig at a livery stable we have to pay in accordance not only with the character of the outfit, but in accordance with the time which we use it. If we ever attain a period of ultimate justice, which not even the most eminent mathematician could attain, such an arrangement might apply to the telephone business; but I think that while we are discussing analogies it might be well to consider this: Think of the injustice which is done to the man who gets into a street car at Bowling Green and rides to Wall Street, when another man may get into that same car at Bowling Green and ride to Harlem. Now, when the tariff on the street railways is so adjusted that the mile ride is the basis of the fare, then such an arrangement may be possible in a talk over a similar territory. Again, it might be said that a two-cent stamp will carry a letter from Eastport, Me., to San Francisco. Think of the unfairness and injustice involved, then, in charging two cents for sending a letter from Eastport to Portland in the same State. I merely mentioned the other simile in order that those who wished to consider the question of telephone rates might have that in mind, not to confuse them, but to aid them.

I once had the privilege of being called in consultation with a most distinguished professor of political economy on the subject of telephone rates. This professor was called in on account of his wisdom and knowledge of such matters. He had previously distinguished himself in the matter of railroad rates and he has since become the president of one of the largest universities in America. After going over the whole subject as well as he might at a conference, he is reported to have said that he found that the railroad rates were very easy, but that the question of telephone rates was beyond him.

Now, my idea in bringing up these matters with reference to rates was not to oppose what has been said to-night, but rather to place alongside of those considerations certain other considerations and merely to emphasize what the author of the

paper has so well said, that in these directions lie engineering and economic questions of the greatest importance, not to be settled in a moment and not to be settled merely by argument, but by resort to facts and the fullest possible consideration.

MR. EDWARD P. THOMPSON:—I would like to say in reference to what Mr. Carty has said this evening that I wish that he would touch upon the progress made in connection with the use of dynamos for telephone circuits. I am particularly anxious to know what has been done practically in that line, and possibly there may be others who would also like a little information on that point. I mean, of course, the current which goes through the transmitter and the telephone circuit.

MR. ABBOTT:—Without endeavoring to create a discussion, because that is the very last thing I want to do, I should like to make one or two comparisons as well. We have just heard a most excellent speech from Mr. Carty, and we know he is a good author, because we had a paper from him some years ago which has been really a monument in both the electrical and the telephonic world. Why has he never written us another paper? Personally, I have learned a great deal from Mr. Carty, and I am sure I could learn a great deal more from him, and I wish he would give me another chance.

With regard to the receiver and the transmitter, it is probable that in the paper I was not explicit enough. I should have recollected my engineering training rather than my economic training and should have gone into details and specifications. To what extent have the changes that Mr. Carty has alluded to actually improved the efficiency of signaling and transmitting? To what extent to-day can we signal more surely or better than we did ten years ago? Will the magneto bells to-day ring better, will they ring louder and more surely so far as the bell is concerned? We will eliminate the question of the power generator at the switchboard, which has been improved; we will eliminate the question of the switchboard apparatus. But to what extent has the ability of the operator at the central office to send a signal and of the subscriber at the sub-station to receive one, been enhanced by the changes Mr. Carty describes? In a similar manner, to what extent have we improved transmission by changes in the transmitter since the original White solid-back? It is very true that in almost all the cities we have substituted what is called a common battery receiver and transmitter for the older forms of high resistance receiver and low resistance transmitter. But has transmission been improved? I think not, to any very marked extent. On the contrary, if I may transcend the professional ethics of which Mr. Carty spoke a few moments ago, and if my memory serves me correctly, the average transmission of the common battery receiver and transmitter is about 80% of that of the local transmitter and receiver when in its best condition. It is true, as Mr. Carty will probably point out in a moment, that the local battery was not of such a nature as to give

constantly 100% transmission, and in that respect and to that extent, transmission has been improved by the substitution to which I think Mr. Carty alludes.

Analogies, like averages, are dangerous. There is no question about that, and I think Mr. Carty uses some which are open to correction. I did not attempt to remodel the ethics of the street car companies nor to correct the abuses of the post office. That they exist is unquestioned. That perhaps the telephone tariffs are no worse, may be true. That, to my mind, was not the subject under discussion, but it was the question of the correction of a particular thing that seemed to need it. Undoubtedly, Mr. Carty could suggest a method which would improve both the post office and the street car service.

MR. CARTY:—A member who spoke just before Mr. Abbott asked in regard to the use of dynamos in connection with the telephone. While that is not immediately concerned in this discussion, I have no objection to saying a few words in answer to him. I presume the question is, how far a dynamo might be used for serving for a number of transmitters at once. That is a matter which occupied a great deal of attention some years ago, and, before that the question of how a number of telephones might be used from one battery was considered; and, while it may now seem strange, yet the fact is that a number of men, then learned in the art, had attempted to use two or more telephones from one source of battery current, and they were met always with this difficulty: When one was talking anybody who had a transmitter supplied by the same current could hear the talk, whether it was intended for him or not. It may seem easy now to those who have all of the facts before them to account for those troubles, but the fact is that many years intervened before we knew what was the trouble. I believe the first time that two or more telephones were run from one battery was an experiment which I made in the Western Electric Company in 1887, when I appreciated the fact that we needed a low internal resistance battery and consequently a constant potential source of supply. It is well enough to talk of constant potential in reference to electric lights when we are dealing with the eye; when we have the retina, the active part of the eye, which will not respond promptly to changes in illuminating power, and furthermore, when we have an electric lamp which will not respond readily to changes in current. When we come to talk of constant potential in telephony, it becomes a question of almost mathematical precision; and, to my surprise, I found that a large number of telephones connected to a few cells in parallel could be used as a supply for a number of telephones operated simultaneously. That was the first practical common battery system ever to be employed. Storage batteries at that time were not so reliable as they are now, and there was a very natural desire to replace the battery by uni-polar machines, which had been suggested, and some work was done in that way. Nothing has come of that, and to-day as a rule dynamos

are used for supplying telephone current for operating transmitters only when the dynamos are bridged by suitable storage batteries, in order, as it were, to smooth out the current and to supply suitable e.m.f., when the commutator is at certain points. Some experiments have been made with special machines with a view to doing away with the storage batteries, but not much has come of it. The gain is not great. I think this will probably answer the member's question.

MR. H. G. STOTT:—I would like to ask another question which I am sure the telephone people think is a very interesting one, and that is, as to the developments which have been made in following out the theorems originally propounded by Oliver Heaviside sixteen years ago, and more recently revived by Dr. Pupin, as to the attenuation of wave form, and so forth, and the means of remedying it. In attempting to read Heaviside's papers, I remember one sentence in particular where he concluded a long series of mathematical demonstrations by saying that "self-induction was the life and soul of telephony." In looking over Dr. Pupin's papers more recently, it seems to me that the whole general subject was threshed out very fully by Heaviside, but I would like very much to hear what progress has been made in following up Dr. Pupin's more practical work.

MR. CARTY:—I regret very much that Dr. Pupin is not here himself. His work, I am proud to say, was the basis of a most important telephone paper, and I think was not recognized as such at the time; but now that we are counting noses on papers I would like to have that mentioned among the telephone papers.

I do not undertake to speak with any authority on the subject of Heaviside or of Dr. Pupin, but I will put my remarks in the form of questions and not in the form of assertions, and in reply to the remarks of the gentlemen, I will ask:

Is it not true that Oliver Heaviside called attention to the importance of introducing into telephone lines self-induction under certain circumstances?

Is it not true that others, as well as the speaker, read Oliver Heaviside's papers years ago and since?

Is it not true that others, following as best they could, the instructions of Oliver Heaviside placed self-induction in the line?

Is it not true that others, having followed and having been guided by Heaviside, failed to produce the result which Heaviside thought should be produced?

Is it not true that Heaviside himself in his works said that he must confess that practical trials had not borne out his expectations?

Is it not true that Dr. Pupin has come along and told us that this self-induction is a good thing to put into the telephone line, if you put it in in a certain way; that self-induction is a bad thing to put into the telephone line if you put it in in another way? In what way should this self-induction be inserted into the line? And, if I recollect right, is it not true that Dr. Pupin

said to put the self-induction into the line in certain definite amounts as he stated, placed in the line with special reference to the wave length?

Is it not also true that he showed us how to determine what the wave length is, and has he not told us which condition we should regard the wave length as representing; that is, whether it represents the line when it has self-induction in it, or before?

I put this in the form of a question, as to whether Dr. Pupin has not told us precisely how to put self-induction into the line, in precisely what relation to the wave length, and told us, as well, precisely how to calculate the wave length. I think that a proper pondering upon those questions might reconcile the difficulty which is in the mind of the questioner, and I know of nobody better able to answer these questions that I have put, than our distinguished President.

PRESIDENT STEINMETZ:—Gentlemen, I suppose I shall have to answer, as you seem to wish to have me do so, and I will try to do it. What Heaviside has done is to show how distributed self-induction inserted in a telephone line improves telephonic transmission by eliminating, as I stated in my introductory remarks, the destructive effect on the wave shape of the line capacity. The line capacity being uniformly distributed along the line, it follows that self-induction must be distributed uniformly along the line. Very considerable self-induction is required, however—too much to make it practicable to insert it uniformly in the line. Trials have been made, then, to insert self-induction locally in the line, either at one end or the other, or at a number of places. The results were discouraging: Where, without self-induction you could hear a little, with the self-induction you could not hear at all.

I do not care to go into the mathematics of the problem, but will try to illustrate the cause roughly on the blackboard.

Let us follow the telephone line from the receiving telephone toward the sending end. At the receiving end a certain current is required to operate the telephone. Following along the line, from mile to mile, to this telephone current—which is slightly lagging—adds itself the current consumed by the line capacity, which is leading 90°. The line current thus becomes leading, first slightly, then more and more. As long as the lead is small, the current is not much increased, since the energy current flowing into the telephone, and the 90° leading current of the line capacity combine to the resultant current as the hypothenuse of a right triangle with the two component currents as sides. With the increase of lead, however, to 80°, 85°, the resultant current increases greatly, and thereby the energy consumed in the line, and this effect is proportional to the frequency, so that ultimately only an insignificant part of the generated high frequency current reaches the receiving telephone.

Prof. Pupin has shown as the result of mathematical and experi-

mental investigations recorded in our TRANSACTIONS, that self-induction, to be beneficial in a telephone line, must be inserted at so frequent intervals, that before the current has reached a considerable lead, that is, before it has noticeably increased, it is changed by passing through the self-induction, to a moderately lagging current, then along the line gradually becomes leading again, but before noticeably increasing, passes the next self-induction, etc. That is, Dr. Pupin has found that the limiting distance, in which the reactive coils must be inserted, must be such that in this distance, for the highest frequency of importance in telephony, the current does not assume such a lead as to increase seriously. If this condition is fulfilled, the current all along the line is approximately a minimum, that is, the energy consumption or attenuation of the power is a minimum.

If, however, self-induction is inserted in wide intervals, as in all previous experiments, while the current has already reached an excessive lead, the only effect is to change the excessive lead to excessive lag. This gradually diminishes and again changes to excessive lead, is at the next reactive coil changed to excessive lag again, etc., but the resultant value of the line current is still just as excessive, whether lead or lag, and the attenuation or loss of energy, therefore, such as to destroy speech. No benefit is therefore derived from the self-induction in this case in reducing the resultant current in the line, except for very low frequencies which may be benefited. This, however, means a discrimination against higher frequencies, or the very thing which is objectionable in telephony, in destroying articulation. This feature, together with the increase of the effective length of line caused by the insertion of a few large reactive coils, is the cause of their harmful effect, observed in previous experiments.

MR. STOTT:—I think Mr. Carty very nicely side-tracked my question, as to what had been accomplished in the way of progress in using Dr. Pupin's system.

PRESIDENT STEINMETZ:—I may say regarding thereto that I am not very familiar with the practical application of the invention, but I do not doubt that the invention is extremely useful. You must consider, however, that some years must always elapse between invention and practical application. It was in the 90's only that the first induction motor was developed, a number of years after its invention. It takes a great deal of time to develop things. You must consider that induction coils have to be designed to give the most effective inductive effect with the least energy consumption. If you consider that the frequencies that you have to deal with probably reach 3,000 or even higher, you see that this by itself is quite an engineering problem, to develop such induction coils suitable for being strung along a long-distance telephone line every few miles or so. I am sorry that Dr. Pupin is not here to tell us more about his work in this direction.

MR. THOMPSON:—I would like to mention some experiments

which I made. I had occasion to test various transmitters, and made some investigations of the powers of different substances. One interesting result was that they seemed to show that it was possible to make a metallic telephone transmitter on the style of the carbon transmitter and yet without producing pressure of the electrodes. That, you may remember, was quite a problem once—to produce a variation of current without compressing the high resistance conductor. Regarding the result, the telephone effect is as loud as if a magnetic telephone were used, but it is not as loud as with a carbon transmitter. Without taking time for details, I will mention only the principle underlying the mechanical construction, which consists in bringing the particles of metal into more intimate contact by a suitable force independent of mechanical pressure, care being taken to avoid compressing the granules between the electrodes. I so constructed a device that the force of independent electric energy directed upon the transmitter could be employed, and in such a manner that when the device was exposed to air vibrations, this electric energy, instead of the pressure, was varied. The particles were metallic and so were the electrodes. Reiss had a hard time in trying to prove that his telephone was operative for articulate transmission, although made of metallic electrodes,and that was the foundation of Berliner's first patent, as you know. It has generally been accepted that we cannot make a transmitter by using metal. Half a word is transmitted, for example, and then the current is broken and the other half is missing. Metal will act as a microphone, more properly speaking. Take a watch and put it in contact with a sounding board, and put a metallic microphone on it, and it will act as a transmitter, but just as soon as air gets in between the original sound and the microphone, we will find it impossible to catch full words in the receiving telephone.

Another experiment showed me that when a magnet was brought near a diaphragm of any microphone transmitter, the sound became fully twice as loud in the receiver. It is almost impossible to get a transmitter in the best adjustment, but with a magnet that adjustment may be made better than by any mechanical process. If the magnet is brought on one side of the diaphragm, it may strengthen it and bring the adjustment right; that is, it will strengthen the microphonic power by putting it on a certain side, and weaken it when put on the other.

It occurred to me to try the lodestone as a substance possibly having microphonic property even greater than that of carbon. By taking a piece of lodestone and bringing an iron point down and pressing upon it, the galvanometer needle will indicate the increase of current to a much greater extent than with carbon. Lodestone, however, would not be as suitable as carbon for a transmitter, because of the difficulty in manufacturing it into any suitable shape, being so brittle. At the same time, it has the property of microphonic variation to a much greater extent

than carbon, although carbon has heretofore been considered, I think, to have the property much more than any other substance. Of course, all substances of high resistance have that power, more or less.

MR. W. J. HAMMER:—The President in his admirable opening remarks stated in substance that up to the present time no telephone relay had ever been made to work. I may perhaps be pardoned forcalling attention to some experiments which I made along while ago in which a telephone relay actually did work successfully. Through the courtesy of the American Telephone and Telegraph Company of New York, which in 1889 extended to me the facilities of its lines from New York to Philadelphia, some interesting experiments were made where a telephone relay was in actual operation. On Feb. 4, 1889, I had the honor of delivering a lecture upon "Edison and his Inventions," before the Franklin Institute, and took three of Mr. Edison's remarkable inventions, namely, the loud-speaking telephone or electro-motograph receiver, his carbon telephone transmitter, and his phonograph, and worked them in juxtaposition, they constituting an operative telephone relay. At the New York end of the line there were stationed at 18 Cortlandt street, speakers and musicians, who spoke, sang and played in front of the phonograph. The phonograph was then placed in front of the carbon telephone transmitter, at 18 Cortlandt street, and the speaking, singing and cornet playing were transmitted to Philadelphia and there repeated by the loud-speaking telephone receiver, and every person in the Franklin Institute heard music. Those who have experimented with Mr. Edison's loud-speaking telephone receiver know that an audience of 5,000 persons can hear it.

At Philadelphia there was a second Edison phonograph which recorded the talking and music coming from the receiver. This second phonograph was then placed in front of a carbon transmitter and the singing and music and talking were sent over a local circuit around Philadelphia and back to the stage of the Franklin Institute. There it was reproduced to the audience by a second loud-speaking electro-motograph receiver.

In that particular experiment, therefore, there were employed two Edison carbon transmitting telephones, two Edison motograph receivers or loud-speaking telephones, two sets of induction coils and batteries, and 104 miles of long-distance metallic telephone circuit, 6 miles of which was underground and sub-marine cable, and 98 miles of which was wire strung on poles. Those who were present at the Franklin Institute on the night of Feb. 4, 1889, will confirm the statement that the apparatus shown constituted an operative telephone relay.

In subsequent experiments that I have made on the other side of the water, in Paris at the Exposition of 1889, and on other occasions, where I repeated those experiments, they were likewise successful. I know that Lieut. Jarvis Patten and others, who have spent a great deal of time and money on telephone

relay work, have stated that the only telephone relay which actually operated as a result of an enormous expenditure of money, time, etc., was the telephone relay that worked on the occasion referred to. I felt that, in view of the President's statement, it should be mentioned that there was at least one telephone relay that had actually worked.

MR. CARTY:—I wish to have the privilege of moving a vote of thanks for the very interesting papers read by Mr. Abbott and Mr. Dommerque, calling attention to the fact that in the question of telephone rates there is an engineering side.

MR. ABBOTT:—I would suggest, Mr. President, that the motion should include Mr. Carty, for I think he has contributed as much, and perhaps more than the others, to the pleasure of the evening.

PRESIDENT STEINMETZ:—The Chair takes pleasure in putting the motion, with the amendment suggested.

The motion was carried, and the meeting on motion adjourned.

(DISCUSSION AT CHICAGO, MARCH 28TH, 1902.—Mr. R. H. Pierce in the Chair.)

MR. F. J. DOMMERQUE:—It is certainly astonishing what a progressive movement the telephone has made in the last few years. During the first eighteen or twenty years of the régime of the Bell Telephone Company, the expansion of the telephone business was very limited. For instance, in Chicago in 1899 there were not more than 22,000 subscribers. At present the number is double that. The same is true of most cities all over the country, except where there is an independent plant. Why should this movement of increase in numbers of telephones have taken place just in the last few years? Personally I can see no other reason than that the Bell Company, during the period when no competitor was in the field, knew that it was cheaper and netted them more revenue, if they had a limited number of subscribers and charged a high rate, than to charge a low rate and have a large number of subscribers, for with a large number of subscribers there is a good deal more labor and expense which has to be paid for. With the appearance of the independent company in the field, the Bell Company, it seems to me, has had to change its policy by reducing the rates and obtaining more subscribers. A great deal is due to the independent movement for such a large increase in the number of subscribers, I am sure. It may be that Mr. Hibbard does not feel the way I do, but I thought it was well at least to call attention to this fact.

With reference to Mr. Abbott's paper. I would like to call attention to a few points: On the third page, after the introduction, the author states: "It is possible to-day for anyone to build a first-class telephone exchange, and with good care, shrewd and expert management, to give service that cannot be excelled, at the lowest possible price. Such, I say, is possible, but, un-

fortunately, it is rarely done. On the contrary, a very erroneous idea prevails in the mind of most of the independent companies as to the requirements and limitations of telephony," etc.

According to this statement, I judge that he means that all they do, in the independent work, is to put up a stick, tie a line to it, and the work is all done and they have a telephone.

I want to call attention to Table II., General Telephone Statistics. At the bottom of that page Mr. Abbott says: "This table is compiled from various sources, and is believed to be fairly accurate within about six months." Furthermore, he says that "B" and "I" signify "Bell" and "Independent" plants respectively. The first city mentioned is Baltimore, where the Bell people have 6,300 telephones and the independent company 6,000. You will observe they are pretty close together in the number of telephones, and I think the independent company did well to get that many where the Bell company had been operating twenty years.

The next city mentioned where the two companies are operating is Cleveland, where the Bell company has 13,744 subscribers and the independent company has 9,000. Again the independent company has a pretty good showing.

This table shows that in Columbus the Bell company only is operating, but as a matter of fact, there is an independent company there also, which has a good many subscribers. The same is true of Indianapolis. There is also an independent company in Pittsburgh with a great many subscribers; also in St. Louis and Minneapolis. Coming back to where the comparison is given between the Bell and independent companies in Cleveland, the Bell company is recorded as having 13,744 subscribers against 9,000 for the independent company. That means 50% more. When you look at the next columns you will notice that the Bell company uses 83,484 wire miles to connect 50% more subscribers than the independent has. The independent company uses 27,000 wire miles, and therefore the Bell company needs three times as many wire miles to connect 50% more subscribers than the independent. You will notice that the Bell company needs 3.2 wire miles to connect, per subscriber, while the independent people need only 1.4.

We will make another comparison. In Rochester, the Bell company has 4,200 telephones in use and the independent company has an equal number. The number of wire miles per subscriber for the Bell company is 3; independent, 1.25. This is not quite three times as much. The independent companies give continuous service in Cleveland and in Rochester at lower rates than the Bell licensees, which is justified, as the independent engineers build their plants more economically than do the Bell engineers, as proved above.

Then again the author says: "Half a dozen of the prominent citizens of some town conceive the idea of a telephone exchange, an ordinance is rushed through the City Council, of which one or

more of the 'prominent citizens' are members, a company is formed; enough stock subscription is raised to pay current office expenses and the labor of erecting a few miles of wire." etc., etc. He continues and at the end says: "Indeed, the preliminary puff is already felt, as is testified by the present lamentable financial condition in Cleveland, for if ever the true epitaph is written over the unfortunate Everett-Moore syndicate it will read, "Died of too much telephone." Why cannot the syndicate see its way clear to part with the telephone holdings, for which there are plenty of bidders? Because the telephone properties are good dividend payers. It was the street railways that caused all the trouble.

On page 6 the author says, "The independent companies are weak in their lack of coherence, feeble in possessing but few exchanges in the larger towns, and almost none in the great cities, and powerless in the toll and long-distance field." Of course, the statement that the independent companies are powerless in the toll and long-distance field is very true, and when the independent people have the toll and long-distance connections, which they will have, then the competition with the Bell company will be very close. I have found that in all the cities where the independent people have started exchanges, the money invested in these exchanges has been put up mostly by the prominent citizens and the best merchants and bankers in those cities. In St. Louis a great deal of the money was invested by the prominent brewers. They are the largest shareholders of the independent company there, and it stands to reason that those men will look out that the money which they have invested in their own city is taken care of. The principal shares of the Bell telephone company in all the prominent cities in the United States are owned by the Boston interests. It is a very hard thing to fight against the local company, if the prominent local people are interested in the independent movement.

As far as investment is concerned, of course at the beginning of the independent movement there was a good deal of speculation, as Mr. Abbott states in his paper very correctly, and to a great extent there is speculation still, but it is only an exception at the present day if the independent movement is not worked upon a proper and sound financial basis. The independent engineers have made many mistakes, but they have shown that they know enough to construct a plant for less money than the Bell company, and the plants give good service; if they did not give good service, the subscribers would not patronize them.

There is a table on page 372 which gives the probable installation cost per station of a telephone plant sufficient to serve 15% of the population. Just preceding this table there is this statement: "It is possible, even in such a city as New York, to instal a telephone plant at a cost not to exceed $60 per sub-station." That price seems very low. The price of sub-station apparatus is given as $15. Now, a telephone for common battery service

ought not to cost more than $9 or $10. and installation $2, making $12. The cost of wire plants (subscribers' and trunk lines) is $21 a subscriber. I cannot very well believe that this is possible in a large city like New York. Furthermore, the author says that the cost of switchboard apparatus for subscribers' and trunk lines is $12.11 and $6 respectively. If we were not to get more than this, we switchboard manufacturers would go out of business, so I think the total charge of $54 is very low, and I think the independent company would not undertake the work at that price in New York; I believe that Mr. Abbott's figures are high enough for small cities.

THE CHAIRMAN:—I notice that Mr. Abbott, in his paper, suggests that the method of charge be based on the number of messages and also on the length of time the subscriber is talking. He suggests the use of a meter, which he says would cost about $3, to record the number of connections and also the total time that the subscriber talks. I suppose he means during the month. I must say that I cannot see how this will give a fair record; that is, how the record would be an indication of the expense to the company of furnishing such service, even if there was a meter which could be installed for this purpose for that price. It would seem to me, offhand, that such a meter would very likely record more where poor service was furnished than where good service was furnished. I would like to hear an expression of opinion as to that from some one who is in the telephone business.

MR. DOMMERQUE:—As far as the cost of the meter is concerned, I think one may be produced for $3, but it seems to me that the whole principle of measuring service in the same way that it is measured in electric lighting, is wrong. In fact a comparison between telephone and electric light service is not the right one.

Those of you who have been in Europe and have traveled on the street cars on the Continent, know the difficulty of the street car system, where you pay according to the distance you are going. If you are going one mile you receive a red ticket; two miles a green ticket, etc. If you are going " through " you receive another ticket. In addition you have to save your ticket, as now and then an inspector will enter the car and will ask to see your ticket. This makes traveling very troublesome. The Continental companies are now introducing systems like those which we have here, and the people like the change, as it makes their traveling much less bothersome. On the elevated railroads here, you pay your nickel and are carried as far as you wish to go, whether one block or a dozen. You never complain if you are charged a nickel for riding one block, the same fare as one pays who rides a dozen blocks. The same thing, it seems to me, should hold true in connection with the telephone business. We want to do a great volume of business and the subscriber should be unobstructed and unlimited as to the number and duration of calls he makes. The idea of the use of a meter at the subscriber's station is not correct anyway, as such a meter

would record every call made, regardless of whether the subscriber obtained the desired party or not, *i. e.*, all busy calls would be recorded and consequently paid for, if the meters were at the subscriber's station. Therefore, it is necessary that the meter should be at the exchange, so that it may be acted upon by the apparatus controlled by the calling subscriber as well as by the apparatus controlled by the subscriber called for. Anyone familiar with the use of such meters can tell astory about them.

ABSTRACT OF DISCUSSION AT MINNEAPOLIS, MINN., APRIL 7TH, 1902.

The Minnesota members of the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS and other gentlemen interested, held a meeting in the Electrical Building at the University of Minnesota on April 7th to discuss the three INSTITUTE papers on Telephony. The discussion was participated in by nearly all of the nineteen present.

Among other points, attention was called to the fact that capacity effects were of much greater relative importance in telephony than in electric lighting or power transmission, electromagnetic induction having very little importance along the line. While the dominant fundamental tones of an ordinary man's voice range around " lower C," having about 132 vibrations per second, and therefore give a frequency comparable with those commonly used for electric lighting and power transmission, the overtones, which give character to the voice, have several times that frequency. Since the current or the quantity of electricity absorbed in electrostatically charging the parallel wires of a telephone or other transmission line is proportional to the frequency, it follows that the components of the current waves due to the high frequency overtones are dissipated along the line much more rapidly than the lower frequency fundamental tones. This effect changes the character of the sound so that the average voice as heard over the telephone is quite different from that heard in immediate converse. This is one reason why it is frequently difficult to recognize a familiar voice over the line and why those who know each other only by communicating through the telephone, find quite different voices when they meet face to face. The blurring of sounds in long distance telephony is due partly to the fact that certain sounds such as " s " and " e " are of very much higher pitch than others, such as " o " and " u," consequently they are obscured in many cases. When the consonants are inferred from the vowel sounds rather than from being heard, the obscuring of some vowels and the emphasizing of others makes it surprising that intelligent conversation can be carried on as far as is common. The apparent contradiction that the telephone girls with voices pitched higher can maintain communication intelligently over lines which give simply a con-

fused roar for men's ears and voices, may be explained by the sharper ears and intuition of the girls, by their greater skill in articulating distinctly and by their care to avoid speaking at a pitch near the natural vibration of the transmitter diaphragm. The character of the voice is affected to a considerable extent in passing through the telephone repeating coils, the higher tones being muffled in this case by electromagnetic induction and by hysteresis. These changes in the character of the voice as reproduced in the telephone become very annoying when it is difficult to prove one's identity and give some justification for the frequent though vulgar preliminary demand: "Who is this?"

Note was made of the more liberal policy of the Bell interests in late years in connecting with independent exchanges on toll lines. In Iowa and probably in other States, interchange of traffic between rival exchanges is compulsory.

In the discussion of upkeep, stress was laid upon the necessity of distinguishing between depreciation and maintenance. Changes of route and of exchange methods amount to more than depreciation. Some copper wire put up several years ago which cost 11½ cents per pound, now sells as junk at 14½ cents. In some places cedar poles last only five years while generally they last ten to twelve years. Some poles erected in 1888 seem to be in good condition to-day.

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

NEW YORK, April 25th, 1902.

The 164th meeting of the INSTITUTE was held this evening, President Charles P. Steinmetz in the Chair. The meeting was called to order at 8.25 P. M.

SECRETARY POPE:—At the meeting of the Board of Directors held this afternoon the following associates were elected:

NAME.	ADDRESS.	ENDORSED BY
ANDERSON, EDWARD H.,	Designing Engineer, General Electric Co.; residence 10 Gillespie St., Schenectady, N. Y.	C. P. Steinmetz. W. B. Potter. A. H. Armstrong.
ANTHONY, JAMES STOWELL,	General Electric Co., 44 Broad St., New York City.	Gano S. Dunn. C. D. Haskins. T. C. Martin.
BADEAU, CHARLES CUSHING,	Switchboard Engineer, Wagner Electric Manufacturing Co., St. Louis, Mo.	J. F. Badeau. F. Schwedtmann. W. A. Layman.
BAILEY, THEO. P.,	Assistant Manager, Chicago Office General Electric Co., 1047 Monadnock Building, Chicago, Ill.	H.M.Brinckerhoff. B. J. Arnold. R. H. Pierce.
BRISLEY, EDWARD BETTS,	Student, Stevens Institute; residence, 102 W. 75th St.; New York City.	A. F. Ganz. W. E. Geyer. J. E. Denton.
CLARK, WALLACE S.,	Engineer, Wire, Cable and Tube Department, General Electric Co.; residence, 13 Front St., Schenectady, N. Y.	C. W. Rice. W. S. Barstow. H. G. Stott.
COLE, WILLIAM WEEDEN,	Vice-President and General Manager, Elmira Water, Light and R. R. Co., Realty Building, Elmira, N. Y.	R. T. E. Lozier. J. B. Cahoon. W. E. Baker.
CUTLER, JAMES ELMER,	Manager of Philadelphia Office, Stanley Electric Mfg. Co., 26 So. 15th St.; residence, Overbrook, Philadelphia, Pa.	W. L. Adams. J. F. Kelly. C. Van Deventer.

EDMONSTON, EDGAR	DAVIS, Electrical Engineer, The Lackawanna Iron and Steel Co., 401 Delaware Ave., Buffalo, N. Y.	C. F. Scott. W. S. Franklin. N. W. Storer.
EKSTRAND, CHARLES,	Superintending Engineer, Brooklyn Cooperage Co., Brooklyn Elevator and Milling Co., and Palmer's Docks, Foot North 6th St.; residence, 74 South 9th St., Brooklyn, N. Y.	W. A. Anthony. F. S. Holmes. F. S. Woodward.
ELLINGER, EDGAR,	Electrical Engineer, George A. Fuller Co.; residence, 215 E. 79th St., New York City.	Henry Morton. W. E. Geyer. A. F. Ganz.
GRAY, CLYDE D.,	Assistant in Electrical Department, J. G. White & Co., 29 Broadway, New York City; residence, 50 Pierrepont St., Brooklyn, N. Y.	H. J. Ryan. H. A. Larduer. G. L. Hoxie.
HELLEBUCK, GUSTAVE J.,	Electrical Engineer, Mediterranean Thomson-Houston Co., 5 Piazza Castello, Milan, Italy.	E. Thurnauer. A. S. Garfield. E. W. Mix.
HINES, CHARLES HENRY,	Superintendent Electrical Department, Lorain Steel Co., 544 14th Ave,. Lorain, Ohio.	E. A. Merrill. M. L. Mora. W. G. Ely, Jr.
HOLMAN, GEORGE ULYSSES GRANT,	General Manager, The Canadian Electric Light Co., Ltd., 12 Laporte St., Quebec, P. Q.	H. W. Blake. W. C. Andrews. T. C. Martin.
HOUGH, BENJAMIN KENT,	Sales Engineer, Stanley Electric Mfg. Co., 1601 Empire Building, New York City.	C. A. Stone. E. S. Webster. J. F. Kelly.
HUMPHREY, CALVIN B.,	Office Manager, Westinghouse Electric and Mfg. Co., 711 Neave Building, Cincinnati, Ohio.	C. F. Scott. Arthur Hartwell. C. A. Terry.
HUNT, CHARLES WALLACE,	President, C. W. Hunt Co., Stapleton, New York.	J. M. Smith. Elihu Thomson. T. C. Martin.
JUDSON, HANFORD CHASE,	Assistant Engineer, General Electric Co., Dobbs Ferry, N. Y.	Eugene Griffin. E. H. Mullin. T. C. Martin.
LEBLANC, MAURICE,	Consulting Electrical Engineer, Westinghouse Electric and Mfg. Co.; residence, Villa Montmorency, Paris, France.	T. C. Martin. J. W. Lieb, Jr. C. A. Terry.
MCNARY, CHARLES HERBERT,	Superintendent of Provo Station, Telluride Power Co., Provo; residence, Nunn's, Utah.	P. N. Nunn. C. A. Adams. S. E. Whiting.
MILES, J. WALTER,	Electrical Engineer, Westinghouse Electric and Mfg. Co., Irwin, Pa.	C. F. Scott. L. A. Osborne. H. W. Fisher.

MILLER, DWIGHT DANA, Salesman, Westinghouse Electric and Mfg. Co., 120 Broadway, New York City; residence, Upper Montclair, N. J.	C. F. Scott. P. T. Brady. E. W. T. Gray.
MURPHY, GEORGE R., Engineer Operating Department, The Electric Storage Battery Co.; residence, 65 W. 93d St., New York City.	G. F. Sever. F. B. Crocker. Aug. Treadwell, Jr
O'DONOVAN, LEO J., Student, Columbia University, residence, 302 W. 72nd St., New York City.	G. F. Sever. F. Townsend. A. E. Poirier.
SHELDON, EDWARD ELLIS, Electrical Engineer, Hudson River State Hospital, Poughkeepsie, N. Y.	G. C. Van Buren. G. A. Redman. W. S. Andrews.
SHORT, SIDNEY HOWE, Technical Director, The English Electric Mfg. Co., 112 Cannon St., London, E. C., England.	W. D. Weaver. T. C. Martin. H. W. Blake.
STORER, SIMON BREWSTER, Engineer and Salesman, Westinghouse Electric and Mfg. Co., Syracuse office, 902 University Block, Syracuse, N. Y.	C. F. Scott. P. T. Brady. P. M. Lincoln.
STOVEL, RUSSELL WELLESLEY, Electrical Engineer, Pittsburg and Lake Erie Railway, Coraopolis, Penn.	J. J. Bellman. Clarence Thomson. L. A. Herdt.
WADDELL, CHARLES EDWARD, Electrician in charge Electrical Department, Biltmore Estate, Biltmore, North Carolina.	T. V. Bolan. Theo. Stebbins. S. J. Smith.
WELLMAN, HAROLD ROBINSON, Electrical Engineer Motor Department, General Incandescent Arc Light Co., 572 First Ave.; residence, 3 Beech Terrace, New York City.	P. J. Kruesi. J. H. Hallberg. M. J. Insull.
WEST, ERASTUS LOVETTE, Assistant to Electrical Engineer, J. G. White & Co., 29 Broadway, New York City; residence, 50 Pierrepont St., Brooklyn, N. Y.	H. A. Lardner. H. J. Ryan. Maurice Hoopes.
WHITE, FRANCIS JOSEPH, Student, Columbia University; residence 53 St. John's Place, Brooklyn, N. Y.	F. B. Crocker. G. F. Sever. A. E. Poirier.
WILSON, NORMAN JAMES, Electrical Engineer, British Westinghouse Electric and Mfg. Co., Ltd., Trafford Park Works, Manchester, England; residence. 804 Wood St., Wilkinsburg, Pa.	C. B. Auel. N. W. Storer. E. M. Tingley.
WOODBURY, DANIEL CORYTON, Student, Stevens Institute of Technology; residence, 934 Bloomfield St., Hoboken, N. J.	A. F. Ganz. W. E. Geyer. J. E. Denton.

YORKE, GEORGE MARSHALL, Assistant Electrician, American Telephone and Telegraph Co.; 15 Dey St., residence, 44 Irving Place, New York City. G. T. Hanchett. A. P. Allen. F. A. Pickernell.

Total, 36.

The following associates were transferred to membership:

Approved by Board of Examiners, March 14, 1902.

WILLIAM CARPENTER WOODWARD, Electrical Engineer, Narragansett Electric Lighting Co., Providence, R. I.

FRANK POWELL COX, Electrical Engineer, General Electric Co., Lynn, Mass.

PRESIDENT STEINMETZ:—Gentlemen, the subject of to-night's papers and discussion is "Electric Elevators, Electric Hoists and Telpherage." The nature of these applications of electricity is that of intermittent service, similar therefore, to a certain extent, to electric railroading; and indeed, both the electric elevator and the electric railroad are solutions of the problem of electric locomotion, the difference being that in the electric railway the motion is essentially in a horizontal direction while in the elevator it is in a vertical direction. There are also intermediaries whereby the two types of locomotion are more or less connected with each other, on the one side in the rack railroad, on the other side in the inclined elevator. From these different kinds of motion follow the differences, as well as the similarities, between the two types of service, and the distinction in regard to the solution to be applied to the problem in the electric railway, which is approximately a horizontal motion, the load on the motors at normal steady speed running, being very small compared with the load put on in special cases, for example, when climbing grades, or especially in starting. This means that the starting torque has to be very greatly in excess of the average running torque. At the same time, however, no limit is imposed on the speed by the problem of railroading, because when a railroad car is without power applied, its normal condition is that of rest. On a down-grade the car may be accelerated, but no grade can exist, that is to say, no grade that may be climbed as up-grade by the car, can be so steep that the car cannot be held against gravity by the torque. In other words, the danger of the running away of a car or train is a consideration of relatively secondary importance in the railway car. Furthermore, the distances are such that the only danger that has to be guarded against is that of collision. The problem with the elevator is different. Here we have motion in a vertical direction, with or against gravity; that means that even if the elevator car is balanced more or less, or over-balanced, we still have a condition where the load, without power applied or without torque applied, is not stationary, but work is done upon it by gravity. Therefore, we have either to work against gravity, that is, do

continuous work in addition to the friction, in retaining the car in steady speed motion, or we have to hold back. Here we still require the excess torque in starting; but since the average load in the elevator is relatively much higher than is the case in the railroad problem, the proportion between starting torque and average running torque of the elevator is much less. That means that while the electric elevator still requires an excessive starting torque, this starting torque is not so enormous as in the electric railway, and less margin is sufficient. At the same time the normal condition of the elevator car without power applied is not rest, but motion. Under acceleration of gravity, it is essential to have some holding-back or speed-limiting device, and for elevator service, therefore, we require a motor having a limited speed, which it cannot exceed essentially, but beyond which it works back as a brake. You see then that there are two conditions imposed on the elevator service, the heavy starting torque and the limiting speed, to a certain extent antagonistic, because the former requires a motor of the type of the series motor, and the latter requires a motor of the type of the shunt motor or alternating current induction motor. The condition of limited speed does not exist in the electric railway service, but the other condition is more severe. We can see that while in their intermittent character, in their general principle of locomotion, of starting and of acceleration, steady speed running and retardation, the two classes of service are fairly similar, still the solution of the problem must lead to the use of different types of motors; in one case to the series motor, a motor with excess starting torque and no limiting speed; in the other case to a motor which is more or less a derivation of the shunt motor, namely, a compound motor; that is, a motor in which a shunt winding with its property of limiting the speed is combined with a series winding, which remains in circuit at starting, thereby reducing the speed, and increasing the torque in proportion to the load, and is usually cut out after steady running has been reached.

Thus far we have spoken of the relation between the electric elevator service and the electric railway service as akin to it. As regards electric hoisting, considerations apply similar to those governing the electric elevator service, except that in the hoist, as a rule, freight only is carried and the speed is very much lower than in the passenger elevator. Therefore, the problem is rather simpler, for not the same absolute safety as regards running away or getting out of control is required as in the high-speed passenger elevator. I may say, however, as regards the comparison between the electric and other types of operating machinery, of which the most prominent is the hydraulic, that the hydraulic has some very important advantages. The hydraulic cylinder and piston constitute a very steady, smoothly operating motor. However, it has the disadvantage of a very low efficiency. With the electric

motor, naturally, the power required by the elevator corresponds to the load, or rather where the car is balanced for average load the power given by the motor is the friction load plus the power corresponding to the difference between the actual load and the average load. The hydraulic cylinder, however, requires the same volume of water at all loads. Whether the elevator is moving fully loaded or is running light, this cylinder must be filled, and therefore the power consumption does not vary with the load. This statement refers obviously to the case where the water is supplied at a constant, definite pressure, say at the pressure of the city water service. Where the pressure of the water may be varied according to the load on the elevator, the hydraulic elevator may be operated very efficiently; but in that case the elevator must be equipped with some motor which varies in power consumed in correspondence to the load, as, for instance, the electric motor. This has been done long ago, and is done now—hydraulic elevators are operated by electric motors, the electric motor merely pumping water to supply the hydraulic piston. That is rather an indirect method, so it is not necessary to dwell on it. The essential advantage of the electric motor is its high efficiency, because the power consumption is proportional to the load only, and there is no power consumption when not loaded, while this is not the case with most other types of motor.

Now, I request Mr. Sever to give his paper on "Power Consumption of Elevators Operated by Alternating and Direct Current Motors." Probably it would have been better, judging from the results of the paper, to say "by direct current and alternating current motors."

MR. GEORGE F. SEVER:—Mr. President and Members of the INSTITUTE: Some time last fall, Mr. Rice, the Chairman of your Committee on Papers and Meetings, suggested that an interesting paper could be prepared on the comparative cost of power for alternating and direct current elevator motors. An investigation was started with the results given in the following paper. The data presented has been gathered by three of the senior students in electrical engineering at Columbia University, and the records have been made with particular care in order that they might be authentic. Undoubtedly some minor errors have crept into the records which appear in this paper, but these will be corrected. We have been very ably assisted by the elevator manufacturers and the manufacturers of the electric motors. The endeavor has been made to present concrete and reliable results.

[Mr. Sever then read his paper. See page 385, April issue].

Discussion.

President Steinmetz:—Gentlemen, you have heard a paper, which, while relatively short, has brought before you a very large amount of material for further consideration and discussion. The showing made by the alternating current motor in this paper is so disastrous that I hope some of the advocates of the alternating current will take up the matter as soon as they are ready for discussion and defend the alternating current in this respect. I might say one thing, however, in its defence. I think it is not quite fair to mass all the alternating current motors. I wish that Mr. Sever had separated the rheostatically controlled motor from the motor with no controller, and had shown us the current of this motor in starting, because there are two essentially different types of induction motor; the rheostatic motor and the motor with no controller. The former motor is essentially a high torque acceleration motor and would be the motor eminently fitted to be chosen for elevator service as far as feasible. This motor should give results closer to those derived from the continuous current motor than the motor without any controller, which, I am sorry to say, has been most generally installed here, giving poor starting torque and large starting current. While Mr. Sever mentions that the rheostatically controlled motor starts more slowly—that is not necessarily the case. In the rheostatic motor a small starting current may be allowed by limiting the starting torque. Larger torque may be obtained by increase of current. There is one point which appears to be rather high, the power factor in starting. I would not have expected a 60-cycle motor to give any power factor like that, especially one of 97 per cent., which is certainly a good deal higher than anything I have heard of for 60-cycle induction motors. The next paper on the program is by Mr. Charles M. Clark on "Telpherage." Mr. Clark, unfortunately, could not be here, and Mr. Burnett has kindly agreed to read the paper in abstract.

[Mr. Clark's paper was then read by Mr. Burnett. See page 391, April issue].

President Steinmetz:—Gentlemen, you have listened to the second paper, on "Telpherage," discussing another problem, in this case horizontal propulsion; not, however, with a car running on a track, but with a vehicle suspended from a track, a wire rope. Discussion is now in order. I will call first on Mr. Ihlder to give us his views on the electric elevator question, with which I know he is very familiar.

Mr. John D. Ihlder:—I beg to call your attention to the fact that Curve 4 refers to a direct current machine of 110 volts. This fact is stated in the paper, but in small print which is apt to be overlooked, especially since all the other data is for 220-volt machines.

The information presented in this paper treats the subject only from the standpoint of power consumption, which in elevator

service is really of minor importance. The most essential requirements of elevators are safety and good operation, and in both these respects the elevator operated by alternating current is inferior to the elevator operated by direct current, because the alternating current motor is fundamentally a single-speed motor, since variation of speed by resistance is not permissible in elevator service where the descending load frequently changes the motor into a generator, while the direct current motor speed may be varied positively over wide ranges; and the stop, the most important factor of all, may be made to perfection with the direct current motor, while the alternating current motor requires that the machine be stopped by means of the mechanical brake only. This becomes very objectionable for higher speeds and the consequence is that no high-speed alternating current elevators are in operation. You will notice that the highest speed quoted in the paper is 180 feet.

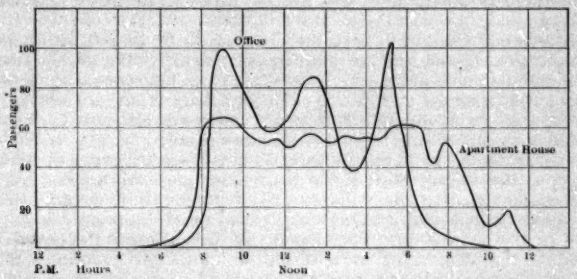
The present state of the art is thus preponderatingly on the side of the direct current motor for elevator service; whether the alternating current motor may be developed to equal its rival must be left to the future.

MR. A. V. ABBOTT:—Mr. President and Gentlemen: It appears to me that the INSTITUTE and particularly those of us who are engineers in consulting practice are much indebted to Mr. Sever for additional information. We all know that one of the most important problems at present is to obtain a good day-load for central stations and the motor is the thing to which we direct attention more than anything else. Elevator motor work appears to be one of the most promising fields which the central station manager can cultivate. Look a moment at the immense number of apartment houses that are springing up in all of the modern cities; in New York for example, I can remember, and I am not as old as some persons here, when there was not an apartment house in the city, and now there are thousands. At first they were only a few stories high, now they average a dozen or more, and some means of transit between stories must be provided; there is no question about that, and so the best means to employ in propelling the elevator car becomes a serious and far-reaching problem and one to which the central station manager may well afford to devote plenty of attention.

It appears to me that the elevator problem may be divided into two parts, in much the same way as the street railway problem. We have the city car which is expected to stop at every block to take on passengers and must have a rapid acceleration, not so very high speed between stations, but must start and stop readily and quickly. Again, there is the interurban service in which the cars are larger and heavier, and run longer between stations, and in which the acceleration need not be so great, but the running speed much faster. Now it appears that the elevator problem is of the same nature. In the office buildings in the business districts of large cities there are two kinds of elevators,

an express elevator, which rarely stops below the twelfth floor, and the way elevator. These two machines are called on for totally different services, as different as those of the interurban car and the city car, and it appears to me that they must be dealt with from different points of view. The express elevator carries large loads and makes a comparatively small number of stops; whereas the way elevator carries smaller loads and makes many stops. The apartment house elevator is usually of the way type and rarely is an express elevator.

I think that the load line of elevator service is well worthy of study and consideration. Two or three years ago I had occasion to deal with an elevator problem of some magnitude involving the consideration of a possible change from hydraulic elevators to the electric service. It was found that the elevator load line would be somewhat as I will show you on the blackboard. Suppose the horizontal axis to be a 24-hour line and the vertical one to represent passengers. There is but a very small load after 7



P.M., and before 8 A.M., but soon after 8 o'clock the number of runs increases rapidly, then drops from 10 to 11 o'clock. There is a peak between 12 and 2, then another peak between 4 and 6. That is a rough diagram of what is to be expected from the ordinary office elevator. In an apartment house the load line is totally different. The load starts at least an hour and a half earlier, runs up very sharply, but remains much steadier during the whole day. There is another peak in the evening, due to the theatre load social calls, and things of that kind. But those load-lines from the standpoint of the designer of machinery to handle elevators, are worth serious and careful consideration. In the office the peaks are sharp, very abrupt, whereas in the apartment the load is relatively much steadier, giving a much broader diagram with fewer peaks. In the office building the load line is more nearly constant from day to day owing to discipline. The large office buildings arrange their elevators to make regular and systematic trips. They have, perhaps, half a dozen elevators; at least two or three are in constant service all the time, so that

the total load line of the building is nearly a constant one, with the peaks of the morning, noon and evening loads superimposed thereon. In the apartment house, on the other hand, the elevator runs on call. It is by no means so constant or so steady a load as in the office building. This point was well brought out in a paper by Mr. B. J. Arnold before the INSTITUTE, some years ago, in which he described a storage battery plant, which he had installed in the Board of Trade building in Chicago. The point that I want to emphasize particularly is the difference in the load lines between the two classes of service. Now that Mr. Sever has broken the ice and has done some missionary work, for which I personally am very much indebted to him, in opening this subject, I think we would all be the more indebted to him if he would go one step further and give us some information with regard to the load lines of the elevators, which he has examined. It appears to me that Mr. Sever is undoubtedly right in calling attention to the fact that too great attention is paid in the apartment house elevator to rapid acceleration. There is no need of it. People do not want it. They do not like it, especially ladies. They do not wish to be jerked up by the root, so to speak, and hurried through the air. The acceleration should be much more easy. Dwellers in an apartment house are not like business men in an office building; they are not in a hurry to get to the 29th floor in three seconds less than no time, but they are willing to go a little more slowly. A man after he finishes breakfast in the morning is not in such a great hurry to get to the office. So that for apartment house work I feel quite confident that a much lower rate of acceleration would prove equally satisfactory and, in many cases, much more agreeable.

In the last paragraph Mr. Sever calls attention to the fact that while the alternating current motor shows on examination a larger expenditure of power, the questions of convenience and of maintenance are important, and I am sure that we would all feel very grateful for additional information upon this point. There are very many cases where we pay more, from the standpoint of power consumption, for the sake of convenience. In fact, I think that a very large proportion of the success of electrical industries has been due to the question of convenience. For example, I have no doubt that we could light this room more cheaply with Welsbach lights than we light it with incandescent electric lights, but we do not. Now the case of an alternating current motor presents one point of peculiar interest in this connection. The average apartment house is not equipped with a skilled engineer. There is an engineer, so-called, but he is very often fireman as well, and in many cases has been promoted from the rank of fireman in an office building downtown, and, while he has a general idea of engines and machinery and can pack a steam gland under stress of circumstances, and possibly patch up a leaky joint in a water-pipe, he knows very little about electrical machinery as such, and the more we can avoid com-

plication in all forms of machinery, which are to be placed under the charge of such a man, the better it is. I venture to say that in the average apartment house the actual expense of running the house for a year would be less with an alternating current motor than it would be with a direct current motor; there would be fewer repairs, less time when the commutators were out of order, fewer broken brushes, etc. We ought to take into consideration not simply the question of cost of power—that is an engineering question—but also consider the kind of attention which the elevator is likely to have.

Apartment house elevators are usually put in the charge of a boy who is anything but an electrical expert, and under these circumstances the machine must run itself. It must operate when it is covered with dust and dirt, when the cellar is flooded, and under all sorts of circumstances, often as bad as those to which the early electric street cars were subjected, when they often ran with brushes half under water. What the owner of an apartment house wants is not simply to know that at the end of the year he has paid for a few kilowatts less, but that his building is a success as a whole, has given him no trouble and his tenants have not complained, and the elevator service has been satisfactory. He does not care particularly for a few kilowatts one way or the other. I think that if we could obtain some information that would show us that the alternating current motor—if that should be the case—was on the whole cheaper when one came to take into consideration cost of repairs, regularity of service and satisfaction to consumers, it would be very valuable.

The President a few moments ago alluded to the use of hydraulic elevators. It is probably true that the hydraulic elevator has been the most successful form of elevator used. But, as Mr. Steinmetz stated, it is a horribly inefficient machine. It takes a whole cylinder of water every time, whether it takes one passenger or an elevator full. Hydraulic elevators are usually arranged so that the descent is managed by allowing the water from the upper side of the piston to flow in on the under side, thus acting as a brake and not necessarily consuming any additional water for the down trip. But, on the other hand, there is no means that I am aware of by which that efficiency may be improved. Inasmuch as the whole cylinder must be filled at every trip, there must be either two reservoirs at different levels or some form of reducing valve to decrease the pressure of the water, or else an equal number of foot-pounds of water will be used whether the elevator is full or empty. So far as I am aware, there are no commercial devices on the market at the present time for improving the efficiency of the hydraulic system. The use of an electric motor for pumping water has been suggested in order to put a steady load upon the motor that supplies power by having a sufficient tank on the top of the building, so that the peaks of the loads could be carried by the reserved water in the tank, and the pump with its motor could operate practically

continuously and under nearly a steady load. In one case I had occasion to remodel some hydraulic elevators and substitute electrically driven pumps for steam pumps, and on the whole in that instance a considerable economy was shown, for the reason that in the ordinary direct-acting steam pump the piston speed is slow, and there is no means of expansion, so the steam consumption is very great. An electric pump driven by a generator operated from a large engine, showed a considerable economy in actual steam consumption and, in that instance, a decided saving.

MR. PHILIPPO TORCHIO:—I think that if we can find any fault with Mr. Sever's paper, it is that he has compared the direct current elevator, which is well developed, with the alternating current elevator which is not fully developed. The comparison would have been more interesting if he had made it on the basis of what the best type of alternating current elevator could do. I was aware that the alternating current elevator used more energy than the direct current elevator, but I am surprised to see that the difference is so large. I want, however, to ask the author how he explains the fact that an alternating current elevator going up takes 50 per cent. more energy than the direct current elevator, and the same elevator coming down takes only 8 per cent. more than the direct current elevator. I do not see clearly why such a difference should exist, unless the elevator tested was designed on considerably different mechanical principles.

PRESIDENT STEINMETZ:—Discussion is still in order and we would all like to hear from Mr. Mershon, who has undoubtedly had considerable experience in the matter.

MR. RALPH D. MERSHON:—Mr. President and Gentlemen of the INSTITUTE: It seems to me that this paper would be more valuable if a little more information were given in connection with it. For instance, in the first table there are a number of things which are not quite clear. One column is headed "duty in pounds." As this paper was prepared with reference to the duty of the motor, that might be taken as the effective weight against which the motor had to work. Again, it might be taken as the load which the elevator is supposed to carry. We have in the next column, "overweight in pounds." Does that mean counterweight or does it mean the difference between the weight of the cage and the counterweight? It seems to me, also, that we might have had a little more information as to the type of motor used. I presume that with direct current it was always a series motor. And we should also have information as to the type and method of control.

There is another point which has already been mentioned, namely, the power factors given for the alternating current motor. They are surprisingly high for the motors on which they were taken—motors without rheostatic control. I do not believe I have ever known of an elevator motor giving as high a power

factor as these are shown to have given at starting. The design of variable speed alternating current motors with variable voltage control is not such as to give a high power factor at starting. The fixed resistance in the secondary is necessarily a compromise between that best suited for running and that best for starting, and it is not that resistance which gives the highest power factor in starting. There was a statement made that the starting torque with a motor having voltage control was necessarily higher, as I understood it, than one having rheostatic control. This is not the case, as a higher starting torque can be obtained with a motor having rheostatic control—quite considerably higher in most cases.

The comparison here has mostly been made between the direct current motor and the induction motor with voltage control. As has been said by one or two others, this is hardly a fair comparison, if we are going to make a general comparison of the direct current motor and the alternating current motor. It may be a fair comparison as between motors which were found actually installed, but the type of alternating current motor of which, for the most part, this paper treats, is the very worst that could be used for elevator work. The last motor given in the table represents the type which ought to be used in all cases and which would give a better result as compared with the direct current motor. The direct current series motor will from the inherent qualities of the motor itself always take less power than an alternating current motor as now built. I say as now built, because I think it is possible to build an alternating current motor which would take no more, and probably less power, than a direct current motor. However, they are not available to-day. The discrepancy between the two motors would not be nearly so great if the comparison were made between the direct current series motor and the alternating current motor with rheostatic control. While the series motor has the advantage as regards the actual amount of power used, it has not necessarily the advantage as regards ultimate economy because we must consider under the question of economy not simply the power used by the motor, but the performance of the motor with respect to the whole system on which it is used. The total number of motors connected in New York City, that is, both constant speed and variable speed, constitutes about 45 per cent. of the total amount of apparatus connected. About half of these motors are variable speed motors. The ultimate question in a case like this would be not as to the relative amounts of power required by direct and alternating motors, but by means of which kind of motor could service be more cheaply rendered: whether for a variable speed motor service which constitutes 22½ per cent. of the total apparatus connected, it is cheaper to provide a means of supplying direct current, or to use for this service a motor somewhat less efficient and by so doing make possible the serving of all the apparatus of the system from alternating current

mains. I think that in most cases the use of the alternating current motor for this work will be amply justified by the cheapening which will ensue in the cost of the service as a whole, due to the fact that it is then possible to supply all kinds of service from the same mains without the intervention of a rotary converter and the expense incident thereto.

There is another point also which we must consider in the question of economy, and which has been raised by some of the others who have preceded me. That is the question of the hardiness of the motor itself. I think there is no question but that the alternating current motor will take the palm on this score.

There was one statement made in regard to acceleration with which I cannot agree, and that was that the economy will be higher with lower acceleration. I think that is not the case. The economy will be higher the higher the acceleration, as far as the power taken by the motor is concerned. Consider a shunt motor controlled by a rheostat in the armature circuit. Part of the current which flows at starting is that for the torque necessary to overcome friction, and part is for the torque necessary to produce acceleration. Now, the latter is greater, the greater the acceleration, and the kilowatts in the series rheostat is greater for greater acceleration. But greater acceleration in attaining a given ultimate velocity means that the current for producing it lasts for a shorter time and *vice versa*, the relation between current and time being such that the kilowatt hours expended upon the rheostat and due to that portion of the starting current necessary for acceleration is the same, no matter what the time taken to accelerate to a given speed, and is always equal (disregarding efficiencies) to the energy stored in the mass accelerated. That part of the starting current necessary for the friction torque is the same no matter what the acceleration; therefore, the kilowatt hours lost, due to this part of the current are proportional to the time taken to accelerate. It follows, therefore, that if we accelerate quickly, while we shall not alter the kilowatt hours lost, due to the acceleration current, we shall lose less kilowatt hours due to the friction torque current. If there were no friction load it would make no difference whether we accelerated in a long or short time; the kilowatt hours lost in the rheostat would be the same, and always equal to the energy stored in the mass accelerated; while with friction present, the sooner we get the mass up to speed and the friction load current out of the series rheostat, the fewer will be the kilowatt hours lost in the rheostat.

I might say, simply as a matter of interest, that the statement just made for a shunt motor under rheostatic control as regards the relative loss with quick and slow acceleration holds for a friction clutch running at constant speed, and used to accelerate a mass and at the same time carry a friction or other steady load. The statements made for a shunt motor will hold for an induction motor under either rheostatic or voltage control and will

hold qualitatively, though not quantitatively, for a series motor.

The point made in regard to the headings of the two columns —data in pounds and overweight in pounds—has another bearing, and that is this, that the power taken in starting is going to depend on the total mass involved, and without knowing this mass one cannot make an intelligent comparison of the different readings given. Certain curves have been plotted by having the energy in kilowatt seconds for 50 feet rise for a certain number of passengers. That might be very different in two elevators, and while the points shown on these curves seem to come pretty well on the curves, so that the masses involved in the different cases were perhaps somewhere nearly the same, the measurement would be very much more valuable if the masses were given, so that one could analyze the measurements and make a close comparison of the different ones taken. I think also, as mentioned by our President, it would have been very much more interesting if we had some of these curves on an alternating motor having rheostatic control.

PRESIDENT STEINMETZ:—Gentlemen, perhaps Mr. Sever can answer a few of the questions raised here regarding the type of continuous motors used, whether series or shunt, and the system on which they were operated, and approximately, what proportion of them were operated direct from electric lighting mains, what from isolated plants, what from separate generators with rheostats reducing voltage and transforming devices.

MR. SEVER:—Mr. President, I can give you some of the information, but I have not all of it immediately at hand.

Taking up some of these points, perhaps a little out of order, Mr. Mershon asked as to the duty and the overweight. Regarding the headings which appear in Table 1, the "duty in pounds," is the weight which the motor is designed to lift. The motor when operating at full load will lift that number of pounds if placed upon the car. The "overweight" is the number of pounds which overbalances the weight of the car. In tests from 1 to 5, as I remember the conditions, compound motors were used, that is, a direct current motor with a shunt and a series winding, the series ampere turns, acting in the same direction as the shunt ampere turns. The method of control in test No. 1 at the Horace Mann School, direct current motor, was that of the Otis Company, known as the magnet control, that is, a magnet which actuates a rheostat arm throwing the current onto the motor and gradually cutting out the resistance in circuit. Test No. 2 at the Mason & Hamlin building was of an A. B. See elevator using a magnet control. Mr. See is here to-night and I think he can explain that test much more fully than I can.

Test No. 3 was at the S. Stein & Co.'s building and was of an Otis elevator with one of that company's motors, which was regulated by hand, that is, a wheel in the car regulated the resistance in the armature of the motor.

Test No. 4 was of an Otis elevator with an Otis motor using

magnet control of that company's style 67, this being somewhat similar to the other type.

The Bryant Park Studio building has an A. B. See elevator with an A. B. See motor, using magnet control similar to the other.

The further tests Nos. 6, 7, 8, 9, 10 and 11 were of alternating motors using, in the last two cases, two different types of control, and in the first four cases simply the switch throwing the current onto the motor. In the presentation of the paper, except unfortunately in one case, I have carefully avoided mentioning the types of elevators and motors. I understand that it is against the desire of the Committee on Papers and Meetings to make such mention, but as the request has been made, I will give these additional details:

Test No. 6 was of an A. B. See elevator with a No. 3 type C, Westinghouse two-phase motor, with no control.

Test No. 7 was of a National elevator, type C, Westinghouse No. 8 motor, two-phase, sixty cycles.

Test No. 8 was of a Reedy elevator, type C, Westinghouse No. 2 motor.

Test No. 9 was of an A. B. See elevator, type C, Westinghouse No. 8 motor.

Test No. 10 was of a McAdams & Cortwright elevator, type C, Westinghouse, No. 2 motor.

Test No. 11 was of an A. B. See elevator, No. 15, type F, Westinghouse motor. The type of control in test No. 10 involved the use of two transformers, one giving 60 volts for starting, and the second giving 175 volts for running, the motor being started first on 60 volts, and after getting up to speed being operated on 175. (When this was operated more carefully by the students who were making the test than it was by the regular operator of the elevator, the currents were considerably reduced). In the last test, No. 11, the elevator was operated with resistance in the rotor, giving small starting currents, and having four points on the control.

I will admit that the power factors on starting are high, but it was very difficult to get instantaneous readings of the current and the voltage on these motors with the alternating current instruments, using a wattmeter, an ammeter and a voltmeter. The alternating current instruments were not dead-beat. Numerous trials and readings were made and the results are correct as nearly as possible, but I will admit that a 97 per cent. power factor is a little excessive. It was quite a difficult proposition to get instruments, or to make the instruments read satisfactorily with the tremendous variations in the current. It was necessary to employ two ammeters, so arranged that the large reading instrument could be used in starting and the small one could be immediately cut in to ascertain the running value of the current. The suggestion has been made by Mr. Mershon that further comparison should be made and more definite data

presented for the different machines. We have all that data and I would like to follow his suggestion and present it as a supplement to this paper in the form of a communication.

I will take exception to the remark of Mr. Abbott to the effect that the direct current motor is a machine that you can injure easily, that is, the present day direct current motor. On the contrary, it is a very hardy machine. As an instance of that, I have in my laboratory an Eickemeyer motor which was presented to us about ten years ago.

PRESIDENT STEINMETZ:—More than ten years ago.

MR. SEVER:—I know that it was two or three years before I went to Columbia and that is about ten years ago, so it must be twelve or thirteen years ago. That motor has been in the hands of over three hundred students in the junior class, and it is one of the finest operating motors that we have in the laboratory. It still has the old brushes that came with the motor and the old commutator, which has simply been sand-papered, and is in just as good condition to-day as when I first saw it when I went to Columbia University, and it has not received particularly good care. As an example of an elevator motor, I think this is an excellent one. My experience with Eickemeyer motors in the shape of railway motors was not quite so pleasant. I had the pleasure of putting in, I think, a dozen of them on a road in my early days in New York City, after having taken instruction from Mr. Ihlder at Yonkers, but I needed further instruction.

I was interested in Mr. Abbott's remarks on express and way elevators which are used in office buildings, but I understand that there is some question as to the advisability of running express elevators. I think there is a certain height of building in which their use may be economical and advisable. Below that height it does not pay to operate them. I would be very glad to hear more on that subject, and I should appreciate it very much, Mr. President, if we could have Mr. See's views on this matter, because I am sure he can give us valuable information.

PRESIDENT STEINMETZ:—We should be glad to hear from Mr. See, but before that there is a question which I think you have not answered—how far the elevators were operated directly from electric lighting mains, whether intermediary motors were used to supply the voltage, or whether they were operated on separate systems, especially with reference to the continuous current motor, because the alternating motors could be operated from the 60-cycle system.

MR. SEVER:—I am afraid that I will have to call upon one of the gentlemen who made the test, Mr. Rosenblatt, to give us that information.

MR. ROSENBLATT:—The only elevator that was operated on an isolated plant was the first one. We had a 110-volt circuit in that case for house lighting, on which the elevator was run. All the other elevators were operated from the street circuit, 240 volts supposedly.

PRESIDENT STEINMETZ:—Was any intermediary apparatus used?

MR. ROSENBLATT:—Nothing in between.

PRESIDENT STEINMETZ:—Mr. See, we should like very much to hear from you.

MR. A. B. SEE:—As engineers, it is proper that we should take up this question of the relative durability of the alternating and the direct current motor for elevator service. Practically, both with the direct current and alternating current, elevator motors are very durable. Plenty of elevators of both types are running here and scarcely any repairs are necessary. Consequently, I think there is very little difference in that respect to the man who is purchasing an elevator.

As for the motor itself, we use a shunt-wound motor with some series turns, and I think that almost everybody uses the same—not a series motor, but purely a shunt motor, with some series turns to help in starting and the shunt to give steady motion. I think that the excess current in starting the alternating machine is due in a great degree to the large armature. In a direct current machine it is possible to have a smaller armature, but in the alternating current motor there is a very large armature, and a great deal more current is required to start that heavy armature than is the case with the direct current motor.

MR. ARTHUR WILLIAMS:—Mr. President, I am very sorry that Mr. Sever's paper shows this large difference in favor of the direct current electric elevator as compared with the alternating current elevator. I have been hoping that the advocates of the alternating current would make a better presentation of their side, showing either that the tests were incorrectly made, or at least that there was some hope of obtaining better comparative results in the future. We certainly want good and efficient motors and elevators of both types.

Curiously, the conditions in practice are exactly the reverse of the theories outlined by a former speaker. On any large system with which I am acquainted, even the largest elevators are supplied directly from the street mains supplying the lighting installations; with very few exceptions the same service will even supply both light and elevator power, and any other power that may be desired, all to an unlimited extent. In the Transit building, to which reference has been made in Mr. Sever's paper, one service is sufficient for all needs. A second service, however, is usually placed in buildings of this nature, to remove any possibility, however slight, of interruption to the service. On the other hand, I think I am safe in saying that it is the practice on alternating current systems to supply elevators and other large power installations from a separate and independent secondary network.

It is true also, I think, that contrary, to the usual expectation and the suggestions of one of the speakers, the alternating current elevator is more costly in repairs, as well as less

economical as regards the current required for its operation. These conditions may seem anomalous, but nevertheless, in the opinion of the operators and the users of the two systems, even additional advantages lie with the direct current—such as higher speed, better control, and entire freedom from noise, either from the motor or the apparatus controlling it.

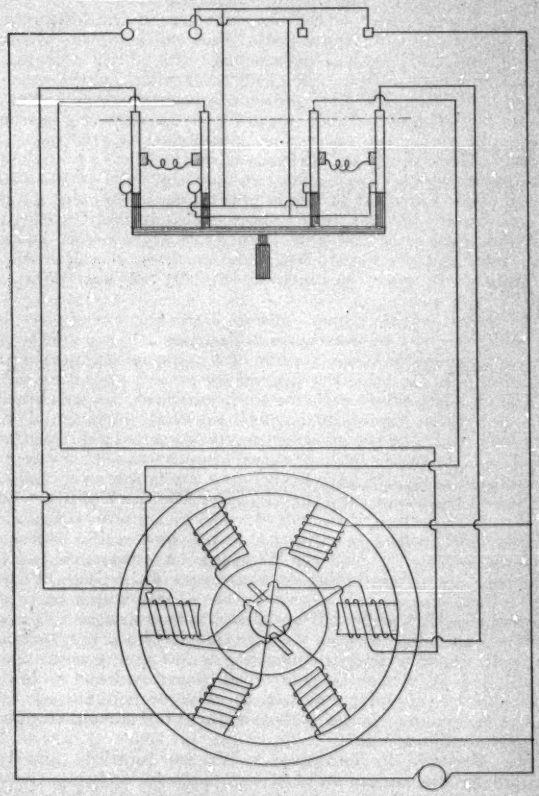
The members present may be interested in knowing in a general way the power requirements of electric elevators of the two types under discussion. Operating with direct current, the direct-connected drum type of elevator with a worm gear—the type in most general use—will consume from 2½ to 3½ kilowatt hours per car mile. When supplied with alternating current for the same work, the current requirements are perhaps a third more. These machines operate within the limits of current consumption which I have given at speeds of from 200 to 350 feet a minute. Elevators of higher speeds, up to 650 feet a minute, which have thus far been equipped exclusively with direct current apparatus, consume from 5½ to 6½ kilowatt hours per car mile, and the electro-hydraulic elevator increases the consumption of current as much as 10 or 12 kilowatt hours a car mile.

In spite of the higher power consumption of the latter type, its use may be found profitable where it is possible to abandon high-pressure steam service and a special engineering staff, because here we have the immediate cost of operating the elevator, in comparison with the unquestionably larger expense of a steam plant maintained solely for that purpose. I think, however, that had the investigation been carried a degree further, the complete substitution of electric apparatus with a direct connected motor would have been found much more economical.

Notwithstanding the little that has been said to-night on behalf of the alternating current elevator and motor, it is a matter of great satisfaction to know that in the more recent types, such, I think, as type "F" of the Westinghouse Company, and types representing corresponding developments of the General Electric and other companies, the results have been much more satisfactory; so that while with motors of former designs it was practically impossible to give elevator service from the systems of the outlying districts, the new types are giving results which enable this service to be given readily and without undue cost, either to the supplying company for a larger distributing system, or to the consumer for an excessive amount of current from which he gets no direct benefit.

MR. EDWARD P. THOMPSON:—We are certainly greatly indebted to the author and his assistants for making these exhaustive tests. I am perhaps among the few to whom it is immaterial whether the alternating current motor or the direct current is ahead in this matter. I think he has shown pretty conclusively as far as he has gone, that as far as the power consumption is concerned, the direct current is probably more economical;

but, of course, as the general view of the members present seems to be that we have to consider which is preferable, I want to speak briefly about a matter alluded to by several speakers in reference to direct current motors, that they involve rheostat



regulation and are therefore objectionable. It reminds me of a case with which I was connected some time ago, involving some investigations by Prof. Anthony. I will give the features of his motor for elevators. It seems to obtain all the essential results

of the proper motor for running elevators, for getting the rapid acceleration and for preventing any jerk or shock to the passengers. The broadest claims were allowed by the Patent Office, thereby indicating that I am not talking about something that does not belong to Prof. Anthony. The principle is illustrated in the diagram.

His experiments related mainly to means for regulating electric motors, and particularly to a regulator for multipolar direct current electric motors. The use of the motor is not limited to any particular purpose, but belongs especially to that class conveniently employed in running elevators, hoisting machines, street-cars, or machinery, the speed of which frequently varies from zero to maximum.

The system involves such multipolar machines as have an odd number of pairs of poles, as six, ten, fourteen poles, etc., since in such machines poles diametrically opposite each other have opposite polarities, and two such poles may be taken as a pair, the magnetic strength of which is to be varied. By so doing, the magnetic attraction on opposite sides of the axis may be maintained in perfect balance at all times, whatever variation may be made in the field-magnet strength for the purpose of varying the speed.

In applying the invention one or more pairs of magnet-coils are cut out in succession, or a pair of coils may be cut out and then cut in again with the connections reversed, thereby reversing the current through the coils. The coils may be cut out as a whole at one operation, or they may be divided into sections, the separate sections being cut out in succession as occasion may require.

The novel conception underlying the motor is the method of operating a multipolar electric motor having an odd number of pairs of poles. This method consists in varying the total electromotive force of the armature by varying the magnetic strength of one or more of the pairs of the field-magnet poles.

Under his demonstration any construction falls that consists of a multipolar motor and means for varying the strength of any pair, or pairs, of field-magnet poles.

Of course it is an old idea to sub-divide field windings and to cut in and cut out field coils, but the practical success of this motor is due to balancing the magnetic attractions on opposite sides of the armature at each degree of regulation.

MR. PHILIPPO TORCHIO:—A gentleman was telling me tonight that some engineer out West called on a manufacturer for some street railway motors and wanted the motors having low acceleration. The manufacturer asked him why he did not ask for rapid acceleration motors and the explanation given was that the conditions there were peculiar as the road was almost always on a level and whenever it was different it was always down.

Now, Mr. Sever has not yet answered my question as to why on going down he has such an efficient alternating current motor,

and going up so inefficient a motor. I would like to hear from him.

MR. MERSHON:—There is a question which I would like to ask Mr. Sever, his answer to which I did not understand before, that is, in regard to the "overweight." I do not understand yet whether that is the counterweight or the excess of the counterweight over the cage.

MR. F. V. HENSHAW:—Since we are asking Mr. Sever for information, I would suggest that he state the acceleration of the elevators described in his paper. It would appear on the face of it, that the acceleration, or period from rest to full speed, was the same in all the elevators tested, but I doubt whether that was the case. Of course that has an enormous influence on the amount of starting current. I would draw attention to the fact that we have discussed thus far the total power required to operate elevators; that is, the power required in kilowatt hours. Now, what is generally of more importance than that (although I must say I have not found many cases of that indifference to meter bills to which Mr. Abbott referred), is the maximum inrush of current at starting, particularly if there is electric lighting on the same mains which supply the elevator motor. A type of motor which does not permit of materially reducing the maximum starting current is not likely to be a very successful elevator motor. The direct current elevator motor is almost universally compound-wound, usually with a great deal of compounding and comparatively little shunt. The speaker who stated a few minutes ago that shunt motors were largely used in elevator work was mistaken. Some of these compound elevator motors work up to 20,000 or 30,000 ampere turns on fields at starting, producing enormously high saturation of the magnetic circuit and they naturally produce a very powerful torque with comparatively little current. I think on account of this low starting current feature that when two sources of electric supply were at hand, one alternating and the other direct, no engineer would hesitate very long as to which he would select for an elevator motor. There is no indication at present that the alternating current motor can be re-designed to accomplish results similar to those obtained by the direct current motor in this class of work.

In regard to Mr. Mershon's view that in discussing the elevator problem the relation of the motor to the system must be considered, it hardly seems to me that this is pertinent to the present discussion. It seems that we cannot discuss the uses of alternating current motors without taking in the whole system. The moment we take any liberties with an alternating current motor, we are liable to mix up the system. The point, however, that I wish to make is that the amount of power required for elevators operated from central stations is hardly sufficient to have a serious influence in determining the method of transmission to be employed. Even if the alternating current motor could be so altered as to make it perfectly adapted to elevator work, I

hardly think that this one adaptation would result in the general adoption of the alternating current distribution in large cities.

PRESIDENT STEINMETZ:—Gentlemen, as regards this inrush of current, I may say that it is indeed a serious matter, especially where there is not a system of unlimited power behind to draw upon. I believe that Mr. Ihlder can tell you something of his experience in the early days when trying to install an elevator motor, in getting the consent of the central station to supply the starting current. Indeed, the alternating current motor is a specially vicious offender in that respect. Perhaps we do not need to blame the motor so much for that. But most continuous current motors involve such an inrush of current that for designing reasons we have to limit it, while this is not essential in the alternating current motor. However, there remains one feature—it is not so much the greater power required by the alternating current motor that is such a disadvantage in elevator service and in a very large part of intermittent service. We really do not care so much for power as for reliability. An alternating current motor is essentially a constant speed motor. It is just as much a constant speed motor as is the continuous current shunt motor. It may be made to run at lower speed by throwing away the power corresponding to the difference between its synchronous speed and that at which it actually runs. The continuous current motor is a variable speed motor. There is no doubt again that the alternating motor *per se* is a more reliable one—simpler in construction—and at the same time we must consider that any apparatus is most reliable and satisfactory and least exposed to damage, when it is run in its proper field of operation. If an alternating current motor, no matter how reliable it is by its inherent construction, is put in an acceleration service, it is probably not as reliable as the continuous current motor, which is probably particularly fitted for that service. That is probably the reason why experience shows that alternating current motors give more trouble in repair than continuous current motors in this particular class of service. At the same time we must consider also that the alternating current elevator motor is not developed yet to anything like the same extent as is the continuous current motor. However, I believe that the question is not whether alternating or continuous current motors are preferable for elevator service at present, because most engineers who have studied that matter will say that they prefer a continuous current motor always when continuous current is obtainable, and where they cannot get continuous current, then the alternating current may be used, and used successfully. They say, too, that it will be still further developed so as to approach still more closely the good features of continuous current motors as used in this particular class of service.

MR. MERSHON:—I agree with Mr. Henshaw that we ought to hear something in regard to acceleration. I disagree with him, however, in the statement that the station has nothing to do

with the question under discussion. It has a good deal to do with it, it seems to me. Suppose you had a plant in which the major portion of the load was lighting and constant speed motors, and with a certain number of elevators to serve. Would you serve them by means of alternating or direct current motors? If by direct current motors, it means that the system must be a direct current system, either in whole or part, instead of all kinds of service from one set of alternating mains. This would involve also the installation of the additional apparatus which will have to be taken care of in order to serve them, whereas, if they were served from the alternating current mains, they would be supplied from the same apparatus that supplied the rest of the load.

MR. HENSHAW:—Perhaps Mr. Mershon misunderstood me. I appreciate fully the force of his remarks, but I think the elevator motor is not a very large factor in determining the method of distribution, being a small proportion of the total load. I think that the paper before us deals with a concrete case of the elevator and its driving motor which can be considered by itself.

MR. MERSHON:—If you let it go at that you leave a very wrong impression, I think. Then the amount of power involved in each case is the all-important thing and represents the relative cost of operation. You leave a very wrong impression as to the cost of operation in ordinary lighting systems.

PRESIDENT STEINMETZ:—I believe that the elevator motor in large cities is of considerable importance in considering the system. From the figures in Mr. Sever's paper, we see that there are about 3,000 elevators operated in New York City. We assume that they are operated at an average of 10 horse power; that amounts to 33,000 horse power of motors installed in New York City alone. I think Mr. Henshaw can give us some information in regard to how important a part of the service the elevator motor represents.

MR. HENSHAW:—I would point out that we do not know how many of these motors are operated from central stations.

MR. TORCHIO:—I think that the subject of the best system of distribution in a city is not involved in the consideration of this paper. I might state, however, that the direct current elevator is a great factor in a large city. For instance, private plants use almost exclusively direct current and direct current elevators. To supply this large class of customers with central station service, the company must be in a position to run their elevators. So the selection of the system of distribution is of great importance. This question was brought out very strongly by Mr. Ferguson, of Chicago, a few months ago before this INSTITUTE. I have only to refer to the discussion of that evening for further information on this subject.

MR. IHLDER:—The question of starting current in elevator work is quite important. We always specify the amount of inrush current which the power circuit must be ready to furnish

for a given elevator and our experience is that there is no trouble on direct current circuits, but on alternating current circuits we frequently are in difficulties, even after the power company has given us the assurance that it is prepared to furnish the amount of current specified.

Regarding the question of robustness of direct current and alternating current motors, it is stated that the alternating current motor requires no starting apparatus and is consequently much simpler, but this is only true when the alternating current motor of highest starting current is used. As soon as the starting current has to be reduced, a rheostatic control must be added and the only remaining difference then is that the direct current motor has a commutator which is replaced on the alternating motor by slide-rings.

About those peaks of power consumption especially in apartment houses, I would suggest that these could be readily made unobjectionable if the size of machine installed were reduced. The usual practice is to install cars and machines for the maximum duty which may be called for a few times per day, probably because the owner believes that the tenants are in a hurry and object to waiting even if only once in a long while.

MR. H. G. STOTT:—I remember one particular case in which a hydraulic elevator, receiving its pressure from an electrically driven pump, cost so much for power that the owners threatened to throw it out. We, however, recommended that an automatic regulating device, which was controlled by the hydraulic pressure, and which automatically started and stopped the motor, should be installed. This was done with a resultant saving in their power bill of about 66 per cent. Without automatic starting devices such as above mentioned, it has been my experience that it is impossible to hold a customer. It has also been my experience that much more reliable service was obtained from the hydraulic-electric driven combination than from the direct electric. We had no A. C. motors in use for elevators.

MR. DOUGLASS BURNETT:—Mr. President, the three gentlemen who have referred to the question of adapting hydraulic elevators to electrical operation have proposed a compromise. They have inferred, while not directly stating it, that existing elevators of this type should continue to be operated by the hydraulic method; substituting, however, the steam-driven, by an electric motor-driven pump. I think the position of the INSTITUTE should be that the direct-geared electric elevator is capable of meeting any building conditions; and that it should be, and is, the sense of this body that the electric elevator is not excelled by any other type. This position is not unfair to the hydraulic elevator, as you, Mr. President, have pointed out that each of the various devices may occupy its own field.

There is, within my personal knowledge, no motor-driven hydraulic elevator equipment in constant daily service in Man-

hattan. There are several such sets operating as auxiliaries for night and Sunday service to systems normally supplied by steam-driven pumps, and a number of them in residences and in other small installations, but neither class is to be considered in this connection.

Within the past few weeks the owners of a Broadway building, eight stories in height, installed a 50-horse-power motor-driven set, replacing steam pumps for the operation of three hydraulic elevators, but it did not pass the acceptance test and is now being removed.

One of the important buildings mentioned in Mr. Sever's paper, was originally arranged for several hydraulic elevators, to be driven in the usual manner by steam pumps, supplied from high-pressure water-tube boilers, which were also to furnish steam for the dynamos for lighting. This plan was changed, electric elevators were installed and are operated with the lighting from the Edison mains and are likely to remain in successful, continuous operation.

There are about ninety cases in this city in which steam and hydraulic elevators have been changed to direct-electric, and the case of a single building is pending in which a number of elevators will be changed in that manner.

The only instance referred to by Mr. Sever, in which the elevators are supplied from a private plant, is the one of which the results of the elevator tests are shown in Fig.4. In that case it would be interesting to show the same kind of curves with the elevator supplied from the direct and constant potential furnished from the Edison mains.

MR. F. H. TAYLOR:—Mr. Sever has admitted in the first part of his paper that the measurements upon which his table was based were more or less inaccurate, owing to the character of the instruments which were available. These measurements are important, and if corrected will change his table entirely. I therefore suggest that his paper be revised for final publication after he has procured from the manufacturers full data respecting the motors in question. I am sure that exact figures are available to him at any time. As an example of what I mean, it would change his table if he found that under no circumstances could the power factors of certain motors exceed 87 per cent., especially if even this figure was too high for the motors under their normal starting conditions.

MR. SEVER:—Mr. President, answering Mr. Taylor's invitation to use more accurate instruments and also to ask for information from manufacturers, I did not mean to imply that our instruments were inaccurate, but that with alternating current instruments, such as we employed and which we knew were correct, it is somewhat difficult to get definite readings when the current is varying so widely and rapidly.

The same elevator was tested numerous times under similar conditions in order to get an average reading on the instrument

for different positions of the elevator. The swing of the needle in an alternating current instrument, as we all know, is considerable. That is, it is not dead-beat, so that the particular reading noted in the test is the average of many and as nearly accurate as it was possible to obtain it.

I thank Mr. Taylor for his invitation to get this information regarding the power factor and I will be very glad to accept it. We have called upon many manufacturers for information, and we appreciate that which has been given us. But there is one point about which we have not been able to get sufficient data, and it answers one of Mr. Mershon's questions; that is, regarding the weights involved. Many do not seem to know how many pounds of metal there are in the moving parts of the elevator system. For instance, the overweight noted in one of the columns has been a very difficult quantity to ascertain. Some of the various manufacturers in answering our inquiries have said they did not know. The way in which they determined the overweight was to put certain weights on the balance and when the current going up was equal to the current going down with a certain number of passengers, I think two, that was the overweight. How much it was they did not know. They simply put on weights until that condition was established. An attempt was made to determine the overweight from electrical readings and some of the results which are given here have been determined in that way. The total weight of the system is therefore not easily obtainable. The power factor may be more easily ascertained. The acceleration is about the same with an alternating motor going up and going down. The tests on that motor were made with a stop-watch, and it is reported that the results were approximately the same. The reason for the smaller difference with the motors coming down is that the direct current motor has to lift the overweight more rapidly, the acceleration being a little greater, and, therefore, takes more power, and the difference between the alternating and the direct in that case is less than in the case of the rising. Of these elevators, 3,300 are operated from central station mains, if I remember correctly. There are one or two in the tests reported, operated from local plants and the elevator in the Horace Mann School is one of these. The kilowatt hours per car-mile have not been ascertained, at least are not given here, but they are available, or they may be calculated from the curve sheet.

MR. WELLMAN:—Since so much stress has been laid upon the starting current required, I think it may be of interest to call attention to one system of operating electric elevators that has been exploited to such an extent that it has been given a sub-class in the Patent Office—the running rope system. This is based on the old principle of the Chinese windlass; an endless rope, forming essentially three loops, one of which passes over an idle pulley by which the car is raised or lowered, and two other loops embrace driving pulleys of two motors. When these are operated

in different directions at uniform speed, or rather the same peripheral speed, the car remains stationary. Then by varying the peripheral speed at either of the motors the car is caused to ascend or descend. By this means I understand that the starting current is kept down to the extent that the inertia of the motor armatures does not have to be overcome in starting the car.

MR. IHLDER:—I believe that the running rope elevator will be found to take about the same starting current as a standard elevator. Though the armatures of the first type are in motion, they still have to start and accelerate the load, and this is done by shunt motors, while the standard elevator motors use heavy compound winding and thus gain sufficient to balance the armature momentum.

MR. SEVER:—I want to correct one impression regarding curve sheet No. 4. The voltage noted there is that across the brushes of the motor and increases as the resistance is cut out, finally reaching the line voltage of 110. It simply shows a rising voltage with the cutting out of resistance.

PRESIDENT STEINMETZ:—Gentlemen, before calling for the motion to adjourn, I believe that, considering the interesting paper and valuable information afforded by Mr. Sever, a vote of thanks would be very proper, extending the gratitude of the INSTITUTE to Mr. Sever and his assistants in his investigations, Messrs. Rosenblatt, Daniels and Haviland.

The motion was made, seconded and carried.

MR. SEVER:—I want to thank you very much, Mr. Steinmetz, for the vote of thanks that has come through you to Messrs. Rosenblatt, Haviland and Daniels, who are here to-night and who have done such good work in obtaining these results. They have certainly gone through a strenuous experience to get them. I thank you very much.

The meeting on motion then adjourned.

DISCUSSION AT CHICAGO, APRIL 25, 1902.

The paper on "Power Consumption of Elevators Operated by Alternating and Direct Current Motors," by Mr. Sever, was read by Mr. Lyman.

Chairman Pierce read written discussions which had been received from Mr. H. H. Humphrey of St. Louis and Mr. S. G. Neiler of Chicago.

MR. HENRY H. HUMPHREY [*Communicated*]:—This paper by Mr. Sever is one of particular interest to me at this time, as I have just been called upon to decide between the use of alternating current and direct current for a factory power plant, consisting of two 75 k.w. units, about 150 rated h.p. of electric motors and about 30 k.w. of incandescent lighting. There are seven elevators in the plant, two of which are at present supplied by direct current from a central station; three are direct-connected steam

elevators, which it is desired to replace with electric elevators; and two are at present belt-driven freight machines. In this particular plant there are good reasons for the use of alternating current motors, but the elevator problem is really the deciding one in favor of the use of direct current.

I have had considerable experience with direct current elevators, but my experience with the alternating current, direct-connected electric elevator has been limited to one machine, and that did not prove satisfactory. I am disappointed that the author of this paper does not discuss the results of his tests more fully, as I find it impossible, from the data given in Table I and the curves he has submitted, to reach any definite conclusion regarding what is the better type of elevator equipment. What I have to say, therefore, is rather in the way of asking for information than adding to the paper.

In the first place, I notice that the direct current machines tested are all of the latest magnet-control type and represent the development of half-a-dozen years in the improvement of starting and controlling apparatus for direct current, direct-connected elevator motors. On the other hand, four of the six alternating current elevators have no control whatever and the comparisons shown in Curves No. 1 and No. 2 are made with all six elevators, which include the four without any automatic control. This is evidently unfair to the alternating current elevator machines.

The next point that occurs to me, is that all the tests are made at comparatively light loads. Only one test on one machine is made at anywhere near full rated load of the elevator, and in that case the current returned to the line on the down trip is not given. It will be admitted, I think, that elevator tests should take into consideration the consumption of power during the entire round trip, and that the full rated load for which the elevator is designed is the only proper loading for testing. When the balanced load is taken during test, the measurements show nothing except the friction of the machinery, the power required for the acceleration and the motor losses. Tests taken with but one or two passengers on a machine, it seems to me, are subject to criticism, as they show little more than in the case when the load is balanced. What is desired is to know what the elevator will do when operating under normal full-rated load conditions.

The high power factor at starting given in the table is surprising, and is a strong point in favor of the alternating current elevator motor. If the power factor is as high as given, why do alternating current elevator motors have such a bad effect in the fluctuation of voltage on the line? It has been found perfectly practicable to operate direct current passenger elevators and high economy incandescent lamps from the same bus-bars in isolated plants where the lighting units are not smaller than 60 k.w., and the passenger elevator machines do not exceed 2,500 pounds capacity at 250 feet per minute. I do not know of any

case, however, where alternating current, direct-connected elevator machines of similar capacity have been operated on lighting circuits from generators of similar sizes in isolated plants. In the case referred to above, a 20 h.p. two-phase motor, direct-connected to an elevator, produced a variation in voltage on a 100 k.w. direct-connected alternator of 6 to 7 per cent., even when the starting current per phase was cut down to 50 amperes at 200 volts. I would like to know why the regulation is affected so seriously if the power factor is as high as given.

The power factor when running is also surprisingly low. The author suggests that this is due to the size of meter used, but as the table does not give the horse power of the alternating current motors, no conclusion can be drawn on this point. Curves 1 and 2 also show that the k.w. when running, by which I mean the k.w. at the middle of the travel between top and bottom of hatchway, is much higher with the alternating current than with the direct current motor. This indicates that the efficiency of the alternating current motor is much lower than that of the direct current machine. This is contrary to my expectations, and may also be influenced by the size.

It is to be noted, however, that if we take the column in Table I, showing the " power in k.w. for running " and confine our reading to those where the load is two persons and three persons, both up and down, the average of the direct current tests is 4.39 k.w., while the average of the alternating current tests is but 2.31 k.w. This indicates that the alternating current machines are more efficient than the direct current machines, and is not consistent with the data given in the curves. It may be, however, that the " power in k.w. running " gives the average k.w. after the first 2½ or 3 seconds allowed for starting was taken off. It would be interesting to know how the " powers in k.w. for running " given in the table are obtained.

It would be desirable to have a careful comparison made between these two types of elevators of equal rated capacity as to loads and speeds, having the same amount of counterweights, and equipped with the latest type of starting apparatus for each type, giving acceleration to the loads in the same period of time for each, and then to have records made under full-load conditions with account taken of the amount of current fed back to the line on the down trips. It is to be hoped that the discussion of this paper will bring forth the results of some such tests.

PROF. P. B. WOODWORTH:—I have had absolutely no experience with the alternating current motor in elevator service, and know nothing about it, but Mr. Sever's paper is quite a surprise to me for a number of reasons; first, because of what I would call poor operation of the direct current motors, as they do not come up to what I expected of them. I have made some tests of the alternating current motor with seemingly very much better results.

In the second place, the alternating current motor, or the in-

duction motor, is the motor that is to take the place of the shunt motor, and, as we all know, the shunt motor is not designed to run an elevator and cannot do so. It is not the proper machine. As I said before, I am surprised to find that the alternating current motor made anywhere near as good showing as this paper indicates. Any type of apparatus where we must have manual control, or where accumulative or compound winding or anything of that kind are necessary—the plain induction motor as we know it at the present time—is not suitable for the work, but it seems to be very much better suited for the work than I had thought.

MR. PIERCE:—In regard to the question of the control of the alternating current motor, as referred to by Prof. Woodworth, it seems that some of the motors used in these tests were plain induction motors, but the other motor, to which he called particular attention, was of a type having resistance in the armature, and this type, as I understand it, would be controllable. In fact I had supposed that there would be even better control than is shown by this paper. I think that Mr. Lyman knows a good deal about this particular kind of control, and I will ask him if he cannot tell us something about it.

MR. JAMES LYMAN:—I would like, Mr. Chairman, to call upon Mr. Lofts, engineer for Swift & Co., who has been investigating this question.

MR. DAVID LOFTS:—We are interested in freight elevator service more than anything else. Our present type is the friction elevator, run by an induction motor, and the question came up as to whether it was desirable to continue to operate this kind of an elevator or adopt a type in which the motor was stopped and started each time that the elevator was used. I did not come prepared with any figures. Tests were made on a passenger elevator, where the lift was 31 feet, the speed per minute 125 feet, and the capacity 3,000 pounds; lever control three-phase induction motor, with external resistance in series with armature. The k.w. hours per thousand pounds per car mile were 15.37. At the works of Swift & Co. a squirrel cage induction motor, running continuously, is used on the friction elevator. With a 64 ft. lift, speed per minute 140 feet, capacity 3,000 pounds, the k.w. hours per thousand pounds per car mile were 16.5. The friction elevator dropped the load and did not consume any current going down.

MR. PIERCE:—Were these figures of the power per car mile based on the round trip in each case?

MR. LOFTS:—Yes.

MR. LYMAN:—I would like to add a word or two to what has been said. The readings that were made on these tests were only brought to my attention this evening, so that I have not made a careful study of them, but the results given in the paper, as has been said before, do not appear to give proper credit either to the direct current or alternating current motor as adapted to the best elevator service.

Prof. Woodworth remarked that the induction motor was similar in its characteristics to the shunt-wound, direct current motor, and he is exactly right, and for that reason the induction motor is not a well-designed motor for elevators. It is capable however, of giving very good results, and the motor on which Mr. Lofts made some tests at Carroll Bros.'s, referred to by him a few moments ago, shows what can be done in elevator practice, taking alternating current from a commercial lighting circuit. In this case there is a 20 h.p. induction motor with resistance in series with the armature and the resistance may be varied by a controller similar to the resistance in a shunt-wound, direct current motor. Readings were taken every 15 seconds for over three hours. During that time, 171 readings showed elevator running, representing about 23 per cent. of the total time during which the tests were made, and during the time of these 171 readings there were only six readings that showed more than about 30 per cent. above full load current of the motor, and those were at the time of starting. The full load current of the motor was 60 amperes. The highest current reading at any one time was 88 amperes. There were only six readings of the 171 readings that were over 80 amperes, and only 30 per cent. of the readings approached full load, although the service of the motor compared favorably with the service of the motor given in Mr. Sever's paper. This motor had a very good controller.

There are three possible ways of running an elevator with an induction motor:

First, it may be connected in the same way as the one referred to at Carroll Bros.'s with an external resistance in series with the armature and a controller, where a most perfect control of speed is obtained, the motor being reversed by the controller.

The second is the method used in most of the tests shown in the paper this evening, where a squirrel cage armature is used. If we examine the tests we will note that the current at starting is from four to five times the full load running current of the motor, and of course this would be disastrous if the motor was connected to any lighting circuit, as it is ten or fifteen times the average running current.

The third method is that used in some of the large manufacturing establishments, like Swift & Co.'s. Here the motor is of the squirrel cage type, but is brought up to speed before the elevator is started, and then the elevator is operated with a friction wheel on a line shaft.

MR. LOFTS:—The current runs about 30 to 35 h.p. maximum?

MR. LYMAN:—The maximum power developed by the 20 h.p. motors running these freight elevators is 35 h.p.

MR. J. W. MABBS:—How is the friction clutch thrown in and out?

MR. LOFTS:—There is a friction wheel, and the winding drum is thrown up against the friction wheel.

MR. MABBS:—What holds the car when a stop is made?

MR. LOFTS:—The weight of the car draws the drum back against a brake shoe. In addition to the motor we have a fly-wheel on the friction shaft.

MR. PIERCE:—I would like to ask Mr. Lyman if, in taking those seventeen readings where the current in no case exceeded the full load current, he thinks any of those readings included the current that was obtained on starting or immediately before starting —that is, the maximum.

MR, LYMAN:—Yes. I think the highest readings included the maximum current taken by the motor during acceleration. It would be difficult to take readings for three hours every fifteen seconds and not strike the maximum. Two indicating watt-meters were used in the test and the ratio of their readings showed reasonably high power factor. In eleven readings, covering a round trip of the elevator, the power factor averaged 88 per cent. There were several other groups of readings where the average power factor ran down as low as 81 per cent.

MR. PIERCE:—I would like to ask Mr. Lyman if he understands how it is that the author of this paper got .79 to .97 power factor in starting, and .14 to .68 with an average of .36 when running. Could any such result as that be obtained where an induction motor with resistance in the armature was used? The power factor would vary in the opposite way, would it not?

MR. LYMAN:—In the reading for the last motor test given in Mr. Sever's paper, the motor was arranged with the resistance in connection with the armature, but I could not see why the power factor should be high at starting. With such an immense current it seemed as if something must be wrong.

I wondered about another thing in looking over this paper. These motors are all two-phase motors, and the current apparently is the current in one phase, while it is compared with the current in the direct current motor. Of course one-half the energy is transmitted to the motor from one phase, so that the current for the two-phase motor should probably be double what it is indicated to be, and it is probably a great deal worse than it looks, as far as the starting current is concerned. It does not seem wise to me to use a squirrel-cage armature induction motor there, when the motor is to be started and stopped while the elevator is running; but it may be used to advantage in ordinary shop practice such as at Swift & Co.'s, where it is very successful, and in riding in these elevators one would scarcely realize what an apparently crude arrangement of regulation they have. The elevators move very smoothly and the motors stand an excessive load.

MR. A. D. AYRES:—It seems to me that the author is comparing the best form of direct current with the very worst form of alternating current; not only that, but wrong sizes. This is very evident, because if we take an induction motor running at anywhere near full load, we get .85 to .95 power factor. I think the author either read his instrument backward or his connections were wrong.

Then, in regard to using a squirrel-cage armature motor; a squirrel-cage armature motor is all right in certain cases, if properly selected for the work, and properly installed. There are two types of variable speed motors—one having a short-circuiting ring where the control is obtained by taking out taps; the other and best way is by inserting resistance in the revolving coils. In that way the starting current will be very small compared with the running current. We have about half-a-dozen of these running now in Chicago, and quite a number are being installed. I think that this whole idea of comparison is wrong, because the comparisons have not been made properly, and it seems to me that the man who made up this report was in error.

MR. PIERCE:—In looking over these tests it seems to me that all these loads were too light to form any basis of judgment. Nearly all the loads were such as would not give a fair comparison.

MR. MABBS:—My experience has been confined to direct current electric elevators, but, judging from this paper, the comparison seems to be very unfair to the alternating current motor. It seems to me like attempting to compare a lumber wagon with the most modern type of automobile. In this case a very crude alternating current machine is compared with the very latest and most up-to-date direct current machine. At the same time it is quite a question whether the alternating current machine is really adapted for the purpose. From this paper it would seem that it is not, and some very important data have been left out—that is, the speed of the car when taking into consideration the starting current. Every one knows that the starting current depends upon the speed at which the elevator is running and the period of acceleration. One may start an elevator very slowly and the amount of current consumed will be very little above the running current. At the same time if that same elevator is started in a very short space of time, the starting current will be very high, and on that account the figures given here ought to give the speed and the period of acceleration.

The author makes comparisons between two different machines. I should judge one of them was running at a speed of 160 feet a minute, and, if I am not mistaken, the other was running at a rate something like 500 feet per minute, and he compares the starting current of one with the other. It makes a great difference whether that car is started up in five, six or ten seconds, or whether it is started in from one to three seconds.

The most important essentials for electric elevators at the present time are high speed and quick acceleration, and the desire is to get these two features and get them with as small a current consumption as possible. That is the problem that is before electrical engineers in the elevator line to-day, and at the present time it has not been very well solved. The direct current machines with which I have had experience take a period of

acceleration of about five seconds, and they make a speed of 400 feet per minute. They take at least twice as much starting current as running current; in fact, sometimes more. I think it is an impossibility to start a car up with 20 per cent. rise of current and fulfil the conditions under which an electric passenger elevator is expected to run to-day. These conditions are pretty nearly the conditions of an old hydraulic elevator. People want it to run 500 to 600 feet a minute and start up in two or three seconds.

The efficiency of the best electric elevator in the market to-day is about 50 per cent. Many of those referred to in this table, I should judge, have an efficiency far below that. The best types of electric elevators that I know of give an efficiency of about 50 per cent. The makers of some of them claim that they will give 60 per cent., and I know of some makers who claim that their elevators will give 75 per cent., whereas they really give but about 35 per cent. These are just plain facts in the case.

In regard to the question of power, I have kept a record of some electric elevators for a period of about five years, and have kept the car miles and the kilowatts per car mile figured up monthly, and what is claimed by the elevator trust to be the best high-speed electric elevator produced to-day takes about 5½ k.w. per car mile under actual running conditions, and the actual running conditions are from an empty car to a load of 2,500 pounds, and running at about 400 feet a minute.

Assuming that the cost per kilowatt in an ordinary plant is something like 1¼c., this would make the cost per car mile about 6.6 cents per car mile under actual running conditions, on the elevator referred to. The k.w. hours consumed were 15.37 and on a basis of 1¼c. would equal 19.21 cents per 1,000 lbs. per car mile. This does not show as good economy as direct current or hydraulic elevators. The best types of hydraulic elevators will run under 3½ c. per car mile, and the question comes down to the point where the electrical engineers must make an elevator that will compete against the best type of hydraulic elevator. If the alternating current is so far below the direct current in efficiency, as shown in this table, and the direct current is so far below what it ought to be, the chances for the alternating current are very slim.

MR. HOBART:—I thought it might be well, inasmuch as the subject of our special elevator was brought up, to say something about the points that led us to adopt that type. Most of the elevators were run by a friction scheme similar to that shown, from a main line shaft. In adopting alternating current in one of our later plants, we put in Crane type motors in some ten elevators, these, however, being only a fraction of all our power. Other conditions in our general plant led us to adopt the alternating current as the best for the whole plant. The elevators were only a fractional part of the system. We found that we had some trouble with the elevator motors—more trouble than

with any other part of the plant. We had lights throughout the plant on the same circuit as the motors, and we began to have considerable trouble with the fluctuation of the lights every time we started an elevator. However, our troubles were lessened considerably when we " slowed down " our elevators from about 200 feet to something like 125 to 140 feet. This led us to look into a system where we could keep the current fluctuation down, and we devised a scheme where we had a large flywheel which took up the fluctuation current. This led us back to our original type of elevator mechanism, and the effect that we wanted to get was even current distribution throughout the plant. The question was an important one, and led us to put in this type rather than the previous type of elevator, controlled by external resistance, starting and stopping the squirrel-cage motor.

Our repairs on the previous type of elevators were at first quite expensive, owing to burn-outs of the squirrel-cage motor. Although we are still running eight or ten motors with the cage and at slow speed, they do not give us a great amount of trouble.

MR. MABBS:—The great fluctuation of load due to starting a high-speed elevator, as has been stated here, has a very bad effect on lighting circuits. In the case of an elevator that takes all the way from 400 to 600 amperes to start, you can imagine that the effect on the lighting system is anything but beneficial. The only way in which we overcome this trouble is by connecting up a storage battery in multiple with the generator which is running the elevator. In that way we can run the elevators with a very much smaller unit and at the same time take the enormous fluctuation off the line. The storage battery responds almost instantly; in fact more quickly than a shunt generator, and supplies the great demand for the first rush of current, whereas an ordinary engine and generator would drop perhaps ten or fifteen volts. Where a generator is run in multiple with the storage battery, the drop will be only about five volts or less.

The most efficient elevators on the market at present are those of the drum type. They start slowly and run slowly. They give the highest efficiency of anything in the electrical line to-day, and the repairs are comparatively small. But the repairs on some of the high-speed electric elevators are very high. This is another question that will have to be solved, that is, a machine that will run at a high speed with comparatively small cost for repairs.

There is still another question, and that is care and attention. The expense in this respect for the hydraulic elevator is exceedingly small in comparison with the present electric elevator. The electric elevator of the future will have to compete favorably with the hydraulic. It will also have to take much less starting current than the so-called high-speed electric elevators of to-day, give a much better efficiency at higher speed, and accelerate much more quickly.

MR. PIERCE:—One point which Mr. Mabbs brought out was the

importance of getting a certain speed and acceleration on electric elevators. He refers to Mr. Neiler's remark where he specifies that the elevator should have a running current not to exceed the starting by 20 per cent. This I believe we have been able to obtain on elevators which will accelerate in three seconds and will run up to 250 feet a minute. We have been able to get it on the same type of elevator for the same service that other elevators will give with such starting currents as are shown in these tables, the difference being in the style of motor used. The motors which are necessary to use to give these results cost more than the motors which give this excessive starting current.

I will say in Mr. Neiler's defence that he has never attempted to use an electric elevator for the kind of service which Mr. Mabbs describes, and I presume that he would agree with Mr. Mabbs as to the advantages of the hydraulic elevator.

The style of motor to which he referred was a cumulative-wound motor, in which the winding of the series field was such that owing to the resistance of this winding and the increased magnetic induction in the motor, the starting rush was held down to about 20 per cent. in excess of full load running current, it being necessary to have the motor specially designed and to have the magnetic circuit such that it could run without sparking. It works on the series field at starting, and in order to work with the full field variation which it has from starting to running, it is necessary to have a machine with a liberal amount of iron, and the machines referred to were designed with this in view, so that they would not spark on varying loads. A machine can be obtained which will operate all right and which will not spark badly by paying an additional price, and that price is justified by taking the load off the system and by the increased economy of running.

MR. H. H. CUTLER:—I have been very much interested in the subject of electric elevators. I have looked into the alternating current motor only sufficiently, however, to form the opinion that it is never used for elevator work except when absolutely necessary, that is, when direct current cannot be obtained, and I have not heard anything said at this meeting that has led me to believe that I was mistaken in that opinion.

Looking at electric elevators from the viewpoint of efficiency, the amount of current consumed is not of very great relative importance. As Mr. Mabbs has said, the demand to-day is for an elevator that will get under way in the quickest time and not only do that, but stop quickly and smoothly. In fact, in this matter of stopping, the direct current motor is far ahead of the alternating current motor. It can not only start more quickly, but can stop more quickly.

We have had a plant in operation nearly two years operated by 40 h.p. motors at a car speed of 300 feet per minute which attain full speed in 3½ seconds. These motors are running on the lighting circuit of a large department store, and we all know that

department store service is extremely severe. We never could have obtained this rapid acceleration unless we had an accumulative-wound motor having about 85 per cent. of series field winding. The elevator specifications for the new Marshall-Field building as originally drawn required bringing the car to full speed in 3 seconds, and difficulty is now experienced in obtaining full speed in that time with that installation. I think that the chief reason for this is that the firm is not using a purely accumulative wound motor, but is using some external resistance which never ought to be employed when a very quick start is desired. We have a great many plants in operation using plain shunt-wound direct current motors. Unfortunately, we have to furnish controllers for everybody's motors, some good and some poor. We do not have much trouble with plain shunt-wound motors, and such motors are used quite extensively by elevator builders. We build quite a number of controllers for plain shunt-wound motors, but they are so arranged that they cannot cut out resistance until the shunt field is fully energized. We have to make our controllers so that they will take care of this. The speed of the motor must be kept constant all the time, and stopped automatically as well as started automatically. I think that we will all agree that this paper must have been written by some one who has not had practical experience with elevators, but who devoted most of his time to measuring the current consumed in the various installations.

MR. E. B. CLARK:—There is still a very large field which I have not heard touched upon to-night, for which an alternating current motor may be adapted—that is, hoists of large capacity for use in connection with blast furnaces and such places. The most important thing about such a hoist is the stopping. There is lots of room to stop, but there is trouble if the stop is not made at the right place, and I would like to ask someone who has had more experience with the alternating current motor than I have had, what possibilities exist with respect to controlling the speed of the alternating current motor for bringing it to a standstill, or for bringing it to a slow speed before setting the brakes for the final stop. That is a very essential point in all automatically controlled hoists with which I have had experience. Mr. Ayres, who spoke in favor of the use of an alternating current motor with the resistance in the rotating part of the machine, will probably be able to answer this question.

MR. AYRES:—It depends entirely on local conditions. If the motor is properly designed, it may be arranged so that it will stop with the controller. In elevators, we do not pretend to stop electrically at all. The trouble that has been experienced before is on account of the fact that too high speed is used. About 6-poles instead of 8-poles are used and consequently a brake will not stop the elevator. The great trouble is that people buy elevators without giving enough information as to where they are to be used, and other conditions.

MR. CLARK:—If I am not mistaken, the introduction of resistance in the secondary is comparable to rheostat control. That is one thing that must be avoided in automatic control.

MR. L. A. NICHOLS:—Elevators are usually contracted for by elevator companies. These companies furnish the motor, and very often they do not go beyond that point. Another fact that comes to me is that the manufacture of the alternating current motor is controlled by one or two firms—virtually one concern; that is, the combination of the Westinghouse and General Electric companies, and the price of the alternating current motor is some two or three times greater than that of the direct current motor. Taking into consideration these facts, the elevator contractor naturally will always put in as cheap a motor as he can get, and very often he installs one that is too cheap, even with direct current.

For such reasons as these, the alternating current motor is kept out of the elevator business almost entirely. I think that there is a prospect for the alternating current motor. After a while other makers will enter into competition. There is no reason that I am aware of, why alternating motors should cost more than direct current motors. I do not see why one cannot be made as cheaply as the other.

Someone made the remark here to-night that it would be a long time before electric power would be as efficient as hydraulic power, but it must be taken into consideration that hydraulic power for elevators will not produce lights.

PROF. WOODWORTH:—I should like to know something about the relative size of the motors, with regard to alternating and direct current.

MR. PIERCE:—As I remember it, that question was answered both ways.

PROF. WOODWORTH:—The way I took it was that the alternating current used, averaged about twice as much as the direct current. However, I may be wrong.

MR. PIERCE:—That was Mr. Lofts's statement.

MR. LOFTS:—I have had no experience with the alternating system, so I cannot answer that question. Under our conditions we had 20 h.p. load, 3,000 pounds, and nowhere near the full load was required to run the elevator.

PROF. WOODWORTH:—I have had no experience with alternating current in elevator work, but in regular service, such as in one case which I remember, we used a smaller size of alternating current motor than we used with direct current, and were able to switch from one to the other.

MR. PIERCE:—Would it be feasible, Mr. Ayres, so to adjust the resistance in the rotor as to hold the starting current down to the running current, with reasonable acceleration?

MR. AYRES:—On such a motor as I refer to, it is impossible to get a very high rate of acceleration, without excessive starting current.

[Adjourned.]

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

NEW YORK, May 20th, 1902.

The 19th Annual Meeting was held this date at 12 West 31st Street, and was called to order at 8.30 P. M. by President Charles P. Steinmetz.

PRESIDENT STEINMETZ:—Gentlemen, Members of the INSTITUTE and Guests: I herewith open the Annual Meeting of the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

The main purpose of to-night's meeting is to receive the annual report of the Board of Directors and also the report of the tellers of the election. The annual report has been presented and distributed. It has been submitted to the Board of Directors at the meeting this afternoon and has been approved. As you will see from this report, the number of members has increased during this year from 1,260 to 1,550, a total gain of nearly 25 per cent., which is more than in any previous year. The total receipts and disbursements have reached the sum of something like $30,000, and although the expenditures of this year have been unusually heavy, due to the expense connected with the library, etc., there is a surplus of nearly $3,000, and if we proceed in the same direction, we will gather a surplus which in the future may be very useful when we desire to secure a permanent home. It appears from the report here that there is a surplus of about $18,000, in addition to a building fund, reserve fund and so forth, amounting to a considerable sum.

I now open the discussion of this report. If you desire to ask any further questions, the committee which has submitted the report, will give you the desired information. The report has been carefully gone over by the Board of Directors and has been approved. Discussion is open.

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

REPORT OF THE BOARD OF DIRECTORS, FISCAL YEAR ENDING APRIL 30, 1902.

The Board of Directors presents herewith for the information of the INSTITUTE a report of its work during the past year, also of the financial standing of the organization.

Certain changes in the administration of affairs have been rendered necessary by the requirements of the Constitution as amended at the last Annual Meeting. On that occasion the announcement was made in the annual report of the Council of the presentation to the INSTITUTE by Dr. Schuyler S. Wheeler of the Latimer Clark Library, and the addition thereto of certain sets of the transactions of learned societies by Messrs. Edward Caldwell and C. O. Mailloux. The cost of the Latimer Clark Library has been duplicated through the generosity of Mr. Andrew Carnegie by the gift of an equal sum, due credit for which has been given in the financial statement. The entire amount has been invested in Government bonds, and about $1,700 has already been expended for the permanent betterment of the Library under the direction of the Library Committee. In order to avoid the sale of bonds, this expenditure was made from the current income of the INSTITUTE and will be suitably adjusted in the accounts.

The Board at its May meeting authorized the employment of an expert accountant to thoroughly examine the books of the Secretary and the Treasurer. This has been done, and by recommendation of the Finance Committee the system of accounts was reorganized upon a double entry basis. No other change was found necessary, as the system already developed was found satisfactory by the accountant in every other particular.

By direction of the Board, the Committee on Standardization was instructed at the June meeting to revise its previous report upon standardization and bring it down to date, with such additions, modifications and amendments as in the judgment of the Committee seemed desirable and proper. This revising work of the Committee has been completed, and the report in its final form will probably be brought before the INSTITUTE at the June Convention 1or final consideration.

At the July meeting of the INSTITUTE the bank account of the INSTITUTE being reported at $12,762.54, it was voted that Government bonds, face value $8,000 be purchased and placed by the Secretary and Treasurer in a Safe Deposit compartment. These instructions were carried out and the bonds named as well as $7,000 in bonds previously purchased, were deposited in the vaults of the Colonial Safe Deposit Co., 220 Broadway.

The annual Convention was held in the New York State Building, on the grounds of the Pan-American Exposition. Twenty-one foreign guests were present, and the sessions of the Convention were well attended and of unusual interest. Arrangements are now being made for the next annual ,convention at Great Barrihgton, Mass., June 18th to 21st, and the various committees are well satisfied with the outlook. Among the general subjects to be presented and discussed are steam turbines, electric railroading and electrical engineering education. A number of papers upon miscellaneous topics will also be presented.

As the room occupied by the Library in the Havemeyer Building was eased by other tenants from May 1st, 1902, the Library Committee in conjunction with the Secretary was empowered to engage other quarters in the vicinity, as it was impossible to obtain the necessary space at 26 Cortlandt Street. Suitable accommodations were accordingly engaged at 95 Liberty Street, one block south from the quarters occupied since November, 1894, and the Library and offices were consequently moved during the first week in May, 1902.

The annual dinner of the INSTITUTE was given at the Waldorf-Astoria on January 14th, 1902, at which Signor Marconi was the guest of honor. Two hundred and eighty-seven members and guests were present.

At the February meeting of the Board, resolutions were passed recommending the adoption by Congress of a bill authorizing the introduction of the metric system by Congress with due regard to the interests of manufacturers.

On April 21st, the INSTITUTE, in conjunction with Columbia University, gave a reception to Lord and Lady Kelvin, which was participated in by the American Society for the Advancement of Science, the New York Academy of Sciences, the American Physical Society and the American Mathematical Society, at which the attendance was about 2,000.

There have been held during the year the Annual Convention at Buffalo, the Annual Business Meeting and nine monthly meetings in New York City, also four local meetings at Chicago and two at Minneapolis. Preparations are being made for local meetings next season at Cincinnati and St. Louis.

The total membership at the close of last year's report was 1,260, classified as follows:

Honorary Members	2
Members	387
Associates	871
Total	1,260
Associates elected May 1, 1901, to April 30, 1902	328
Total	1,588

Resignations have been received and accepted during the year from the following members and associates who are in good standing:

Members—Charles H. Haskins, Clark C. Haskins, Giles Taintor, F. T. J. Haynes, Leigh Carroll and R. E. Dunston.

Associates—W. C. Peckham, E. C. Sharpe, Emile Berliner, James E. Denton, W. M. Stewart and William Wallace.

There have been the following deaths during the year:

MEMBERS:

A. LANGSTAFF JOHNSTON JOHN WARING.

ASSOCIATES:

FRED. W. WOOD, RICHARD R. BRYAN, CHARLES LEMON,
ARTHUR H. WOOD, JAMES H. LORIMER, C. WALDRON SWOOPE.

Total Deaths	8
" Resignations	14
Dropped as Delinquent	7
Elected but not yet Qualified	10
Total	39

Leaving a total membership of 1,549 on May 1st, 1902, a net gain of 239 This exceeds the net gain of the previous year by 212.

The membership is classified as follows :

Honorary Members	2
Members	405
Associates	1,142
Total	1,549

A list of associates elected during the year accompanies this report, and the names appear in the printed TRANSACTIONS.

The reports of the Secretary and Treasurer show in detail the financial standing of the INSTITUTE at the close of the fiscal year, together with an itemized statement of the receipts and disbursements during the entire year.

The financial report is in slightly different form from previous reports, having been prepared by an expert accountant, and is based on the distribution of accounts as they now appear in the ledger

ASSETS.		LIABILITIES.	
Cash	$ 1,385 17	Library Fund	$ 7,455 84
Bonds, market value	16,286	Building Fund	1,100
Library	7,799	Reserve Fund	4,000
Transactions (volumes)	3,286	Compounded Membership Fund	3,600
Office Furniture and Fittings	212 75	Surplus	18,464 76
Badges	80 50		
Catalogue Type and Cases	191 82		
Congress Books	480 00		
Accounts Receivable	4,299 86		
" Secretary Petty Cash	500		
	$34,620 10		$34,620 10

STATEMENT OF RECEIPTS AND DISBURSEMENTS,

FOR YEAR ENDING APRIL 30, 1902.

Receipts.		*Disbursements:*	
Entrance Fees	$ 1,830	Stenography	696 65
Current Dues	13,561 82	Stationery and Printing	830 24
Past Dues	1,298 41	Postage	797 70
Advance dues	135 10	Express	231 01
Life Membership Fund	600	Conversazione	427 41
Transactions Sales	875 49	Badges	297 48
" Subscriptions	434 15	Expenses General	465 27
Binding	32	Certificates	30 75
Advertising	738 59	Office Fittings	67 82
Conversazione	280 85	Salaries	4,318
Electrotyping and Engraving	24 65	Engraving and Electrotyping	713 90
Badges	405 23	Publishing Transactions	4,135 62
Transfer Fees	266	Rent	1,459 08
Congress Books	2 10	Advertising Commissions	186 75
Building Fund	100	Binding	390 90
Interest	372 80	Meeting Expenses	1,064 91
Library Fund	7,335 62	Office Expenses	107 92
		Library "	2,342 33
		Library Fixtures	635 49
		Bonds	8 730
	$27,900 81		$27,808 63

The total receipts for 1902, exclusive of donations to the Library Fund, were $7,459.15 in excess of 1901.

The total disbursements for 1902, exclusive of Bonds purchased, were $7,369.02 in excess of 1901. Of this amount, $2,877.82 is directly chargeable to the Library, making the total of disbursements for 1902 over 1901 $4,491.20.

Due from Member's account of Entrance Fees and Dues	3 859 20	
Unpaid Bills, none		
Total Cash Receipts	27,990 81	
Less Contribution to Library	7,335 62	20,655 19
Total Cash Disbursements	27,808 63	
Less Bonds Purchased	8,720	19,088 63
Gain during year		1,566 56

Property on hand according to inventory May 1, 1902

Office Furniture and Fittings	212 75	
Badges	80 50	
Transactions	3,286	
Congress Books	490	
Printing Material	191 82	
Library	7,799	
	12,050 07	

TOTAL NET ASSETS.

Cash	1,385 17	
" Secretary Petty Cash	500	
Bonds, Market value	16,385 00	
Inventory (as above)	12,050 07	
	$30,320 24	
Total Assets last year	15,375 76	
Increase		$14,844 48

This increase is accounted for as follows:

Contributions to Library (Cash)	7,335 62
Increased value of Library	6,184
Surplus from years' business	1,324 86
	$14,844 48

Respectfully submitted,

RALPH W. POPE,

Secretary.

TREASURER'S REPORT.

FROM APRIL 30, 1901, TO MAY 1, 1902.

GEORGE A. HAMILTON, TREASURER, in Account with AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

Dr.

Balance from April 30, 1901	$ 1,451 48
Received from Secretary, April 30, 1901, to May 1, 1902	27,742 32
	$29,193 80

Cr.

Disbursements from May 1, 1901, to April 30, 1902, on warrants from Secretary, as approved by Committee on Finance and Board of Directors, Nos. 1358 to 1555 inclusive	$27,808 63
Balance to New Account	1,385 17
	$29,193 80

By instructions of the Board of Directors, Government Bonds have been purchased during the year at a face value of $8,000, upon which a premium of $720 was paid. This amount is included in the above disbursements.

Resepctfully submitted,

GEO. A. HAMILTON,
Treasurer.

PRESIDENT STEINMETZ:—If there is no discussion, we will proceed to the next business of the meeting, which is to hear the report of the tellers regarding the election of officers. I call upon the Chairman to give us the report.

The Committee of Tellers, W. J. Hammer, Chairman, F. C. Bates, J. J. Bellman, S. D. Collett and F. E. Kinsman, reported the results of balloting as follows:

FOR PRESIDENT.

Charles F. Scott	501	C. T. Hutchinson	3
C. O. Mailloux	7	F. A. C. Perrine	3
M. I. Pupin	4	L. B. Stillwell	1
W. L. R. Emmet	3		

FOR VICE-PRESIDENTS.

Bion J. Arnold	486	Calvin W. Rice	6
C. O. Mailloux	484	J. W. Lieb, Jr.	5
Schuyler S. Wheeler	467	A. E. Kennelly	4
W. D. Weaver	33	H. J. Ryan	3
C. S. Bradley	16	Townsend Wolcott	3
Wm. Stanley	11	J. J. Carty	1
L. B. Stillwell	11	W. L. R. Emmet	1
A. J. Wurts	10	W. E. Goldsborough	1
Louis Bell	6	C. F. Scott	1
F. A. C. Perrine	6		

FOR MANAGERS.

Townsend Wolcott	501	J. S. Peck	4
Gano S. Dunn	492	L. B. Stillwell	4
E. H. Mullin	462	H. A. Wagner	4
C. A. Terry	458	W. J. Hammer	3
R. T. E. Lozier	42	G. T. Hanchett	3
F. A. Pattison	16	J. W. Lieb, Jr.	3
C. C. Chesney	10	T. C. Martin	3
W. B. Potter	10	P. C. Hewitt	2
A. J. Wurts	7	L. B. Marks	2
H. G. Stott	6	R. N. Baylis	1
Calvert Townley	6	C. T. Child	1
Max Osterberg	5	P. G. Gossler	1
H. J. Ryan	5	Robert Lundell	1
F. V. Henshaw	4	C. O. Mailloux	1
P. M. Lincoln	4	W. L. Puffer	1
L. A. Osborne	4	Fitzhugh Townsend	1

FOR SECRETARY.		FOR TREASURER.	
Ralph W. Pope	518	George A. Hamilton	518

PRESIDENT STEINMETZ: Gentlemen, you have listened to the tellers' report on the result of our annual election. I believe I voice the sentiment of all by saying that we can congratulate ourselves on the personality of the President we have selected, whose

name and general reputation offer a sufficient guarantee that the next year will be prosperous and successful.

In connection with the election, I desire once more to draw your attention to the provision of the new constitution, excluding associate members from the presidency and vice-presidency, and I desire to remind those members who are fully qualified to become full members but who have not complied with the formalities, to apply for transfer. All those who are entitled to full membership should make application.

This business being over, I call upon the Secretary to make an announcement.

SECRETARY POPE:—At the meeting of the Board of Directors this afternoon, the following associates were elected:

NAME.	ADDRESS.	ENDORSED BY
ASH, WALTER VOORHIS,	Chief Electrician, Raritan Copper Works; residence, 79 State St., Perth Amboy, N. J.	W. S. Brewster. E. G. Elliott. R. W. Pope.
BERRY, A. HALL,	Manager of Electrical Department, H. W. Johns-Manville Co., 100 William St., New York City; residence, Montclair, N. J.	Joseph Sachs. Calvin W. Rice. Geo. F. Sever.
BROWN, WALTER EVERETTE,	Central Office Assistant to Superintendent, N. Y. & N. J. Telephone Co., 81 Willoughby St.; residence, 763 Nostrand Ave., Brooklyn, N. Y.	W. J. Hammer. Alex. MacFarlane. J. C. Reilly.
BROWNE, WILLIAM HENRY,	Treasurer, General Manager and Director, Stanley Instrument Co., Great Barrington, Mass.	Wm. Stanley. C. W. Rice. F. A. C. Perrine.
CONWELL, WALTER LEWIS,	Westinghouse Electric and Mfg. Co., 120 Broadway, New York City; residence, Upper Montclair, N. J.	S. L. Nicholson. P. T. Brady. C. F. Scott.
DARBY, WALTER RAINES,	Salesman, General Electric Co., 44 Broad St.; residence, 18 Summit Ave., Westfield, N. J.	F. C. Bates. J. J. Mahony. G. F. Sever.
DAY, CHARLES,	Engineer of Works, Link-Belt Eng. Co., Nicetown; residence, Germantown, Philadelphia, Pa.	R. D. Lillibridge. Louis Bell. T. V. Bolan.
DODGE, KERN,	Electrician, Link-Belt Eng. Co., Nicetown; residence, Germantown, Philadelphia, Pa.	R. D. Lillibridge. Louis Bell. T. V. Bolan.
DOUBRAVA, HARRY WILFRED,	Engineer, Bullock Electric Mfg. Co. and Wagner Electric Mfg. Co., 220 Broadway, New York City.	R. T. E. Lozier. W. B. Spellmire. R. B. Owens.

FARMER, ELMER ELLSWORTH, Engineer of Experimental Department, Stanley Electric Mfg. Co.; residence, 105 Wendell Ave., Pittsfield, Mass.	F. A. C. Perrine. C. C. Chesney. John F. Kelly.
FROMHOLZ, ANTON JOHN, Electrician in charge, U. S. Navy Yard, residence, 1177 Greene Ave.,Brooklyn,N.Y.	Frank Martin. H. Spochrer. S. L. Nicholson.
HARISBERGER, JOHN, Assistant Superintendent, Snoqualmie Falls Power Co., Snoqualmie Falls, Washington.	Eugene Maxwell. S. Z. Mitchell. T. A. W. Shock.
HARRIS, WILLIAM AUGUSTUS, Manager and Engineer, The Suburban Tel. Co., Port of Spain, Trinidad.	Wm. Maver, Jr. Townsend Wolcott R. W. Pope.
HODGKINSON, FRANCIS, Mechanical Engineer, The Westinghouse Machine Co., 411 Colonial Building, Station D., Pittsburg, Pa.	C. F. Scott. L. A. Osborne. W. L. Robb.
HUNTINGTON, DAVID L., General Manager and Chief Engineer, The Washington Water Power Co., Box 1587 Spokane, Washington.	Calvin W. Rice. Samuel Sheldon. J. A. Lighthipe.
JONES, WALTER J., Engineer of Electric Department, Gas and Electric Co., of Bergen Co., 114 Main St., Hackensack, N. J.	W. J. Hammer. F. A. Scheffler. R. W. Pope.
KELSCH, RAYMOND STERLING, General Superintendent and Engineer, The L. R. H. & L. Co., 4093 Tupper St., Westmount; residence, Montreal, P. Q.	R. B. Owens. Samuel Insull. R. D. Mershon.
LUCAS, JAMES CLARENCE MERRYMAN, District Manager, Bullock Electric Mfg. Co., 8 South St., Baltimore, Md.	R. T. E. Lozier. W. B. Spellmire. Frederick Saxelby
LYNCH, JOHN COOPER, Traffic Engineer, New York Telephone Co., 15 Dey St.; residence, 104 W. 71st St., New York City.	J. J. Carty. B. Gheradi, Jr. I. W. Smith.
MOORE, ROBERT HAWTHONE, Assistant Electrical Engineer, La. Pur. Expo. Co., Mechanical and Electrical Bureau, World's Fair, St. Louis, Mo.	G. F. Sever. E. F. Schurig. W. F. White.
ORBELL, ROBERT HUGH, Electrical Engineer, British Westinghouse E. & M. Co., Paines Manor, Pentlow Cavendish, Suffolk, England; residence, 724 Whitney Ave., Wilkinsburg, Pa.	J. S. Peck. P. H. Thomas. C. E. Skinner.
PALMER, WILLIAM HENRY, JR., Electrical Engineer, Assistant to President, New York Transportation Co., 49th St. and 8th Ave.; residence, 154 W. 106th St., New York City.	J. J. Bellman. H. B. Coho. W. B. Spellmire.

RAYMOND, EDWARD BRACKETT,	Electrical Engineer, General Electric Co., Schenectady, N. Y.	C. P. Steinmetz. E. J. Berg. H. G. Reist.
RITCHIE, THOMAS EDWARD,	Business Manager, Royce Ltd.; residence, Didsbury, Manchester, England.	Wm. Maver, Jr. Townsend Wolcott R. W. Pope.
SCHMIDT, HENRY FREDERICK,	Student, Columbia University; residence, 351 West 14th St., New York City.	T. C. Martin. C. P. Poole. W. D. Weaver.
SEMENZA, GUIDO,	Chief Electrical Engineer, Edison Co., of Milan, 4 Via Paleocapa, Milan, Italy.	L. B. Stillwell. C. P. Steinmetz. C. L. Edgar.
SIEBERT, ALGERNON T.,	Experimentalist, Pyro Electric Co., 124 William St., Orange, N. J.	M. J. Wightman. R. W. Pope. Townsend Wolcott
SPALDING, PHILIP LEFFINGWELL,	Engineer, The Bell Telephone Co., Philadelphia, Pa.	Carl Hering. J. J. Carty. Theodore Spencer.
STEINLE, MAX TH.,	Electrical Engineer, with Charles J. Bogue; residence, 317 W. 22d St., New York City.	C. J. Bogue. E. R. Knowles. Max Osterberg.
STORKE, HENRY LAURENS,	2d Vice-President, The Empire State Telephone and Telegraph Co., 102 Genesee St., Auburn, N. Y.	C. T. Child. J. J. Carty. F. A. Pickernell.
THOMSON, GEORGE HUNTINGTON,	Chief Engineer, American Elevated R. R. Co., 25 Broad St., New York City; residence, Ossining, N. Y.	H. C. Wilson. W. J. Hammer. R. W. Pope.
WESTINGHOUSE, GEORGE,	President Westinghouse Electric and Mfg. Co., Pittsburg, Pa.	C. O. Mailloux. Louis Duncan. Elihu Thomson.
WILDER, STUART,	In Testing Room, Stanley Electric Mfg. Co.; residence, 23 E. Housatonic St., Pittsfield, Mass.	D. B. Rushmore. C. C. Chesney. F. A. C. Perrine.
WINN, JOHN EDWARD,	Superintendent of Meter Department, The Cincinnati Gas and Electric Co., 220 W. 8th St.; residence, 1701 Race St., Cincinnati, Ohio.	George Hafer, Jr. J. H. Hallberg. R. W. Pope.

Total, 34.

PRESIDENT STEINMETZ:—Gentlemen, next on to-night's programme is the reading of two papers on topics referring to electric railroading, by Mr. de Muralt and by Mr. Danielson, both relating to European experience, results of the investigations of European engineers and work done abroad. Both refer to the use of induction motors for electric railways. I call first on Mr. de Muralt to read his paper.

[See page 411, April issue.]

PRESIDENT STEINMETZ:—We have listened to a very interesting paper by Mr. de Muralt. The next paper on the programme is

by Mr. Ernst Danielson of Sweden, our member, which I shall abstract here. It also deals with the application of alternating current induction motors to electric railroading and especially discusses the problem of acceleration and retardation and of variable speed. It theoretically considers the solution of the difficulty usually encountered with induction motors, that they are suitable only to operate at one definite speed—the speed slightly below synchronism, and inefficient at any other speed. It is well known that where two equal induction motors are available, by connecting the induction motor in concatenation—" cascade " as our European friends call it—that is, in tandem connection or, in other words, by connecting the secondary circuit of the first motor to the primary circuit of the second motor, this couple of motors operates with efficiency at half speed; that is, the first motor running at half speed produces in the secondary a current of half frequency, the secondary current always being of the frequency of slip, that is, half frequency where the speed is half synchronism. This secondary current impressed upon the second motor makes it rotate at half speed also, so that by using two equal motors in parallel and in concatenation or tandem connection, we get the same effect as by the series parallel control of continuous current motor, that is half speed and full speed. Mr. Danielson's paper discusses the problem of securing more than two speeds by utilizing two motors separately and in concatenation, the motors having different numbers of poles, that is, different speeds. As an illustration, using a 4-polar motor and a 10-polar motor, connecting the 4-polar motor only, it will operate at a speed corresponding to 4 poles: that is, with a 50-cycle per second frequency at 1.500 revolutions. Using only the 10-polar motor, it will operate at a speed corresponding to 10 poles; that is, 600 revolutions. Using now these two in concatenation, we get a speed corresponding to the sum of the poles of the two motors or we get a synchronous speed corresponding to a 14-polar motor; that is, about 430 revolutions. Mr. Danielson shows that a fourth speed can be secured by such a pair of motors by connecting them differentially in concatenation, or so that one of the motors rotates against its torque. In this case, then, the resultant speed corresponds to the difference in the number of poles; in the present instance to 10–4, or 6 poles, that is, 1,000 revolutions. In this manner, by using two motors of 4 and 10 poles, we can get 1,500, 1,000, 600 and 430 revolutions, by utilizing either one motor separately or the two motors in cumulative or differential concatenations. Instead of using two motors of two different numbers of poles, the paper states as obvious that two equal motors can be used and the difference of speed secured by different gear ratio, as, for instance, 4-polar motors with gear ratios 4 and 10.

The second part of the paper considers then the inefficiency of acceleration of the induction motor, compared with that of

the continuous current series motor under the different conditions of operation; by plain ordinary parallel operation of the induction motor or rheostatic control, without any concatenation, thereby using two induction motors of equal speed in concatenation, the ordinary method of getting two speeds; and furthermore by using two induction motors in the previously described manner, so as to get four speeds. You see the table on page 450. This is rather interesting. It shows when considering only acceleration, that the continuous current series motor, straight through, is superior, in the power it consumes for doing the same work, to the induction motor, to bring the car up at a given time to a given speed; when using electric braking, that is, when using concatenation, to return power to the line in the induction motor, the table shows that the continuous current motor is superior to the rheostatically controlled alternating current motor, but the concatenated control by using three or four different speeds gives an advantage in power over the ordinary series parallel control of the continuous current motor; that is, it consumes less power to bring the same car in the same time up to the same speed from rest. This refers, however, to the theoretical case of certain efficiencies assumed for the two motors, and assumes that the efficiency varies with the speed at the same rate, which is not exactly correct, because the induction motor falls off in efficiency for low speeds more than the series motor, since the loss in the iron increases with decrease of speed in the induction motor. But this aside, it considers only the power input, not the current input. In addition to those powers compared here, the paper states, as we all know, that there is a wattless component of current, a wattless power, which is not considered in comparison, which exists in the alternating but not in the continuous current motor.

This gives the general contents of Mr. Danielson's paper, which is extremely interesting, since it brings from an engineer of very high reputation, a very fair comparison. I may say that Mr. Danielson is very familiar with alternating current work and has been identified therewith for many years, and is an engineer of high reputaton in the United States, where he has been a number of years, as well as in Europe. He brings what I consider a fair comparison of the two types of motors, when considering the induction motor under conditions as favorable as possible, that is, arranged so as to give full efficiency at four different speeds.

[See page 439, April issue.]

Discussion.

President Steinmetz:—I may say in regard to Mr. de Muralt's paper, I heartily endorse his statement that we cannot draw general broad conclusions regarding the superiority of one type of motor or the other type of motor, but that each individual railway case has to be considered separately on its merits and investigated, before deciding whether an alternating or continuous current motor is preferable for that particular purpose. For many years the alternating current motor has been developed here in the United States, to such a point that we could perfectly trust it to do the work of running the trolley car, or propelling the railway train, and three-phase induction railway motors have been operated for the last eight years.

By all the better engineers, every individual case has been considered on its merits and has been carefully investigated, as those railway engineers who are present can tell us, to determine whether continuous current or alternating current is preferable, and I hope that in the near future we may see some alternating current railway plants go into operation, although I am sorry to say that thus far there has been no case found showing sufficient superiority for the alternating current induction motor to warrant its installation. I would like to call, however, on Mr. Mailloux, to lead the discussion. Mr. Mailloux, as you all know, has given this matter very great attention and has been in Europe to study the various three-phase plants, and I understand when he came back from there he was very enthusiastic regarding the desirability of a polyphase induction motor for electric railway work. I call on Mr. Mailloux.

Mr. C. O. Mailloux:—Mr. President and Gentlemen: I was hoping that somebody else would open this discussion; I came to listen rather than to speak. I think that I ought to correct the President. I believe I remarked at the Buffalo Convention that when I returned from Europe I was very highly saturated with the alternating current feeling, but that I afterward became demagnetized to some extent, and those who participated in the Buffalo discussion will remember that I showed proofs of the fact that I was somewhat demagnetized by insisting rather strongly on the cases where the alternating current motor was not particularly suited. I have had the pleasure of seeing, examining and riding on nearly all of the electric railroads which are mentioned in this paper.

I was quite struck with the Stansstad-Engelberg road and with the Burgdorf-Thun, as representing examples of what I might call real steam railroading conditions to some extent; more perhaps, by the size of the cars and the peculiar traffic conditions under which they operate. More especially is this true in the case of the Burgdorf-Thun railroad, where they operate trains of two or three cars and apparently handle them exactly under ordinary steam railroad conditions. But there is one

thing that impressed us as Americans, and that was the time required in acceleration. In this connection I must differ radically with Mr. de Muralt in regard to the conclusions that he mentions on pages 434 and 436, as to the rate of acceleration. It is certain that I did not observe any rates of acceleration such as are quite familiar to us in practice in this country. Moreover, the curves which he gives on page 433, as well as the figures which he cites, do not indicate it. He states on page 432 that a 32-ton car was brought to a speed of 24 miles an hour in about 32 seconds. We would scarcely call that acceleration at all here. It is only three-quarters of a mile per hour per second. It is good enough for steam railroad conditions, but it certainly is not what is necessary and essential for good electric railroad conditions. We think that acceleration might be called acceleration when we reach one mile, and it begins to look interesting when we have one-and-a-half or two miles per hour per second. On many street railway lines, like the Broadway cars here, we will find the rate of acceleration going up to three or four miles per hour per second. I will admit that then it begins to become too interesting and to become uncomfortable. At the same time there is a great difference between one mile per hour per second and two-and-a-half or three.

I think the paper is excellently written and gives a very intelligent resumé of the work which has been done there. The illustrations themselves are especially interesting, because they give one an idea of how things look. The data which the author has given are all very useful to the engineer, and really the only conclusion in which I must disagree with him, is in relation to acceleration. That is, however, a very vital point, which discriminates against the use of the alternating current in many cases in this country. In fact, in most cases where the alternating current has been considered and has had to be set aside, it has been due to the fact that the cost and the difficulty of acceleration with the alternating current motor are too great. It is in many cases not a question of cost, but a matter of physical possibility or impossibility, as the case may be. In the discussion at Buffalo I think I went quite fully into the differences between the two kinds of motors, and I considered the reasons why it is not possible to obtain as rapid acceleration with the alternating current motor as with the direct current motor in the present state of the art; hence I do not think it is necessary to repeat them here. In substance it is due to the fact that while the alternating current motor is limited in the maximum torque which it can produce, which slightly exceeds twice or two-and-a-half times the running torque, yet, with the direct current motor we can obtain 4, 5, 6, 7 and even 10 times the running torque. That is the physical principle which lies at the bottom of the whole matter. It is really the thing which enables us to accelerate faster with direct current motors; and where the stops are at all frequent, where the schedule speed is at all high, we find it necessary to reject,

at least in the present state of the art, the alternating current motor. The Burgdorf-Thun line is especially well suited to the alternating current, because the schedule speed is not so very high, the distances between stations are relatively long, the stops are relatively few and the headway is large; all these being conditions which favor that system. In this country when we begin to do transcontinental business with electric railroads, then will the day of the alternating current motor come. When we pass from the purely passenger railroad traffic problems to those which border upon the same service conditions or which are intended to comply with the same service conditions as the so-called steam railroads; then we shall, I think, of necessity have to look to the alternating current motor, because of the many advantages it possesses, which advantages have been quite clearly set forth by the author in his paper. There are many cases to-day in this country where railroad engineers are confronted with problems whose solution they hesitate to undertake because the requirements are such that they really think it desirable to have the qualities of both kinds of motors. In other words, either the art has not advanced far enough, or else the requirements have not changed enough to determine a definite preference for one against the other. This is particularly the case with steam railroads that have through or long-distance traffic and that have also local or suburban traffic. The two parts of their service represent different problems, and it has been found so far that the two are incompatible with each other in so far as they cannot be solved by the same means. The local passenger traffic, the suburban business, usually dictates—I use the word advisedly—the use of the direct current motor, while, on the other hand, the through or long-distance passenger traffic points quite strongly toward the alternating current system. Consequently, either we shall be compelled to use both systems, at first, when we begin to invade the steam railroad field in earnest, or else we must perfect the alternating current system still further, so that it alone may, perhaps, be enabled to fulfil the requirements of both problems successfully and satisfactorily.

I was greatly interested in the paper of Mr. Danielson, which I think is very suggestive of what might be expected in the evolution of methods of using the alternating current motor. I have myself given the matter a great deal of thought, feeling that something ought to be done in that direction. I have myself sought to attain the same result in a different manner, such as by using two or more sets of polar windings having different numbers of poles, in the same stator frame, though in that case there are difficulties in construction. But the idea is substantially the same, and like Mr. Danielson's, it admits of a much greater elasticity in the range of speed and the control of efficiency, as well as the starting torques obtainable, than is possible by the present method, including the ordinary method with rotor resistance, or the method of concatenated or cascade connection.

I very much hope that more will be done, that others will devote thought to this subject and that we shall in a very short time discover many other useful and ingenious methods of enabling us to accomplish with the alternating current motor that which we now find we can do only with the direct current motor.

As I said at the beginning, I have become somewhat demagnetized in my enthusiasm. At the same time I cannot help feeling that there is a great future for the alternating current motor, and I look forward to its evolution and development, and I expect much more from it than has been accomplished hitherto, it being my conviction that it must accomplish a great deal more before it can be looked upon as a satisfactory, all round, general solution of the problem.

MR. HENRY G. STOTT:—I was very much interested in the details of the 10,000-volt trolley line. I think if the trolley did not make very good contact and an arc occurred, there would be a very good opportunity for short-circuit. Do such conditions ever arise? That is to say, assuming the lower trolley contact is imperfect, the arc would certainly rise and strike other wires. Another point that occurred to me was in comparing the method of operating trains with direct current, the number of sub-stations necessary with alternating current work. Here apparently it is necessary, for the sake of keeping the drop down to 10 or 15 per cent., to have a sub-station every three or four miles. In our practice, rotary stations are usually placed not more frequently than from six to ten miles, which would in itself be a considerable investment.

The photographs show the regular Siemens type of horned lightning arrester. Under conditions in this country, this device would last just about one discharge, and I would like to learn of the experience abroad.

PRESIDENT STEINMETZ:—I may say, that considering these European plants, it is interesting to compare somewhat the magnitude of the problems solved there, with American electric railway practice; on the Gornergrat railway the power of the locomotive is 180 h.p. That is very little more than in an ordinary trolley car in New York City, of which hundreds are running. If we turn to the Burgdorf-Thun railway, which is operating under steam railway conditions, we see that even in acceleration the current maximum is 200 amperes at about 750 volts, which corresponds to something like a little over 400 amperes at 500 volts. I have seen an ordinary interurban single trolley car when climbing up a long grade run with over 800 amperes for a considerable period. That is nearly twice the power which the whole Burgdorf-Thun railway, under steam railway conditions, takes for acceleration. For instance, on the trolley road between Schenectady and Albany, running single cars, fairly large cars indeed, the total motor capacity per car is four 50 h.p. motors, or 200 h.p. rated capacity, which is more than the Jungfrau locomotive. In climbing the long grades which we

have to mount, the car takes over 800 amperes, by a single trolley wheel. The construction which is customary in Europe but not in this country, is the long roller trolley, which has the disadvantage that it must be operated in an almost vertical position. That means that the trolley wire must be at an approximately constant distance from the track, and this is not feasible here. We have to cross steam railroads where the trolley wire has to be raised very high, and we have to cross under roads and viaducts where the trolley wire has to be lowered, so we must have a trolley construction where the trolley can receive current from the wire irrespective of the height of the latter. That makes this European trolley construction impracticable. As soon as we come to a trolley pole on a very small angle from the horizontal, or the ordinary trolley construction, it is almost impracticable to use more than one trolley at high speeds, 30, 40 or 50 miles per hour, without danger of destroying the overhead construction every time the trolley jumps the wire at speed. That is a great disadvantage in any polyphase motor, where we must have at least two trolley wires.

Mr. de Muralt's paper is extremely interesting, as I said before, in that it brings out the necessity of considering each individual case on its special merits and also that it shows that three-phase railways can be operated successfully for years without any difficulty from breaking down. However, when considering continuous current and three-phase current, what we have to determine is, does the three-phase induction motor in the particular case offer sufficient advantage over the continuous current system to compensate for the very great disadvantages in the induction motor in railway work.

One disadvantage is the necessity of using more than one trolley pole, which means that with overhead construction the problem becomes very difficult at higher speeds. A further great objection to the present type of induction motor is the speed limit, that it is limited to synchronous speed. In consequence thereof, you cannot get the benefit of the higher speeds which the continuous current motor can reach on level tracks. It is impossibile to make up time, where you have to stop a number of times or where you have an unusually large load, or you have to provide in your schedule that the synchronous speed is very much in excess of the schedule speed, which means low efficiency in operation. Furthermore, the fluctuations in load are necessarily much greater, because you climb hills at the same rate as you run on the level track, while the continuous current motor slows down on a hill, and so saves power. The Burgdorf-Thun motor at 40 per cent. overload starts successfully on a 2½ per cent. grade. That would not at all suffice for our railway practice. It may happen that a train gets stalled on a grade greater than 2½ per cent., say 5 per cent. or 8 per cent. grade, and has to wait for the next trolley car or train to rescue it by pushing it ahead, and that means you have to start from rest at a much greater

grade than 2½ per cent., and at the same time to push a disabled car. That is an overload which is about three or four times the ordinary load the trolley car has to carry.

The general experience in designing a three-phase induction motor railway plant and a continuous current railway plant, is that in continuous current line construction you have to figure on the average drop of voltage in the line, because the drop of voltage in the line affects only the speed and the efficiency, and not the available torque, and in this case only the average drop comes in and excessive drop for a limited time only is not objectionable, since the schedule speed is not lowered seriously by the excessive drop of voltage occurring at some far distant point, by two cars starting on a grade or anything of that kind. In an induction motor proposition, however, you have to calculate the line construction for the maximum drop of potential which may occur in emergencies, because the motor torque goes down with the square of the voltage, and you must under any conditions be sure that even at the maximum drop of voltage, the car is able to start on a grade with an overload, and there we find in practice, especially when considering the lagging current and the drop of voltage due thereto, that the induction motor is at a very serious disadvantage compared with the continuous current series motor, even considering the use of a much higher voltage for the former. Usually, also, the transformer capacity required with an induction motor railway, even allowing for temporary excessive overloads of the transformer, is very much in excess of the generator capacity and the maximum load of the system.

In general, the direction of electric railroad development here is toward higher schedule speeds and more frequent stops, and also interlinkage with the urban systems, so that the same car runs between cities and through cities, and this requires a type of motor for interurban service which can operate on existing city lines. That is very much against the induction motor.

However, the main field of the induction motor would be where you come to a traffic so infrequent that you cannot reach it without going to a very high voltage on the trolley line, 3,000 or more, and where you have your own right of way, moderate speeds, and a very long distance with very few stops. There the three-phase induction motor may possibly compete successfully with the continuous current motor, also on roads where extremely high speeds are reached on your own right of way, 100 miles or 150 miles per hour; and furthermore, in freight traffic over mountain ranges, for instance, to pick up a heavy freight train or coal train at the foot of a mountain, and instead of breaking it into pieces and putting in pushing locomotives, to pick it up by a powerful electrical locomotive, pull it up the mountain range and let it go down again on the other side, while it returns power to the line. There the induction motor would be decidedly superior to the continuous current motor by returning power to the line. You could do it there, because you naturally operate

at low speed, so that you can go beyond synchronism on a downgrade without danger of the train getting away from you. In ordinary railway service, 40 to 50 miles an hour on level track, that is with a synchronous speed of the induction motor at least 40 to 60 miles an hour, you can be sure that it will hardly be possible to run on the motor brake, and going down hill you cannot permit the speed to be high enough to return power.

Now, as I said before, I hope to see in the not distant future, some individual cases where the induction motor is suitable. I may say, however, that if anybody ever developed an alternating current motor of the same characteristics as the continuous current series motor, that is the single phase motor, which has no limiting speed but a torque, which is a maximum in starting and which decreases with the increasing speed, then this alternating current motor would be the motor for railways, even if the efficiency and power factor should not be very high.

MR. W. N. SMITH:—I would like to ask the author what was the average headway or the average distance between stops on this Burgdorf-Thun line, with trains in regular service; also, what is the average drop in voltage which is found to result in ordinary practice, and what has been found to be the lowest practicable drop under the conditions at which that road operates.

In the case of that road or any similar road in Europe, has the use of the rails as one leg of an alternating three-phase circuit been found to unbalance appreciably the circuit, or introduce any difficulties whatever, due to the fact that the conductor is iron and is liable to introduce i nductive influences that do not exist in copper trolley wires?

Another question is as to the durability of those long roller trolleys compared with the life of the wheels in use in this country. I suppose the average life of a trolley wheel properly cared for might be taken as 5,000 miles in this country and perhaps more. I have known of their running that mileage.

PRESIDENT STEINMETZ:—I would like to add here another question, whether Mr. de Muralt can give us some information regarding the life of a trolley wire; whether the use of this roller contact increases or reduces the life of the trolley wire. Gradually the trolley wire is worn down, and I have seen from some city roads where there was very heavy traffic, some pieces of wire worn down about 40% of the section until broken. I believe two million cars with their trolleys had passed along that wire before it was so worn and broke.

MR. W. J. HAMMER:—I would like to ask Mr. de Muralt a question regarding the Zossen Railway.

I had the pleasure of making a personal inspection of all the roads (with one exception) mentioned in his paper, and had the honor of presenting the results of my investigations in a paper[1]

[1] "Important European Electrical and Engineering Developments at the Close of the Nineteenth Century."—TRANSACTIONS, vol. xviii, p. 47.

before the INSTITUTE at its 151st meeting on February 29, 1901.

If you will refer to Figs. 15 and 16 of Mr. de Muralt's paper, you will note that the trolley arms, which make this sweeping motion, are capable of adjustment. There is also an arrangement inside the cab permitting the movement by the operator of the three trolley arms simultaneously, and they are also each independently under tension, so that the upward pressure of the ordinary under-running trolley is practically reproduced by a side-sweeping or wiping motion of the trolley arms together with the lateral movement arranged for in the sections of trolley wires.

The arrangement used in the original plant on the Teltower road between Grosse Lichterfelde and Zehlendorf, which permitted of the flexibility in the contact arrangement between the trolleys and the trolley wires, and which also enabled the keeping of the three trolley wires the same distance apart, and keeping them under the same tension in cold and warm weather, consisted of heavy weights placed inside certain of the latticed poles, which weights, acting through a loose pulley and block with six pulleys, gave a strain of 1,500 kilos on the wire. These poles were about 40 metres apart, and they were somewhat heavier than the intermediate poles, and were set in a cement foundation.

The question which I desired to ask Mr. de Muralt was whether this arrangement, which seemed to me of considerable importance, was also employed on the Zossen road.

In connection with the concluding remark of President Steinmetz regarding the wear upon the trolley wires, and his statement that in foreign practice it was necessary to have the trolley conductors at almost a constant level throughout the entire length of the road, on account of the difficulty in securing adjustment with the foreign types of trolleys, where the trolley wire varies in height, as is frequently the case in American practice, I think that both of these matters are not as serious as one would be inclined to think.

One form of bow trolley which I have seen considerably used abroad, has a groove in which oil is placed, a slight amount of which is constantly keeping the surface between the trolley and the wire lubricated. In other cases the trolleys are so designed as to enable immediate replacement of wearing parts, the principal wear coming upon the trolley itself.

The various forms of trolleys permit of considerable up and down adjustment.

I had the pleasure of seeing the tests made on Messrs. Ganz & Co.'s road at the Alte-Ofen Island near Budapest in 1900, which led up to the installation which has recently been put in by this company on the Lecco-Colico-Sondrio-Chiavenna road in Italy.

In the installation at Alte-Ofen, the locomotive was equipped with a trolley which was raised and lowered by means of compressed air—the same device being used which operated the air-

brake, and which also was used for forcing the solution in and out of the liquid rheostats. This ingenious arrangement saved the necessity of raising and lowering the rheostat plates. The single and double bow contacts are in use on the Swiss roads, and I saw them used upon one road in connection with the American type of trolley pole on certain equipments. There are certain advantages in the bow type of trolley in simplifying the difficulties of overhead construction, and there is not the same danger of the trolley jumping the wire, as is the case with the American type. The type of triple trolley which I have referred to, used on the three-phase road near Berlin, gave most excellent results. On the underground road in Budapest the trolley is adjusted so as to make a contact on the track rail, fastened to the low roof of the tunnel, and upon emerging from the tunnel to make contact with the higher overhead wire.

MR. TOWNSEND WOLCOTT:—I should like to ask one question: What is the average air gap in the polyphase induction motors as compared with the diameters of their armatures?

MR. FREDERICK V. HENSHAW:—The operation of great railroad systems by electricity instead of steam power is a subject that is very much before us and before the public at present, and the public is on the *qui vive* to see which railroad is going to make the first move. It is unquestionably a fact that express trains on steam railroads operating over long distances with few stops can be driven successfully by means of the induction motor, great distances and direct current at the present time being impracticable. The necessary acceleration can unquestionably be obtained by putting on enough motors; if necessary, you can put a motor on every axle. But it is worth remembering that the bulk of the traffic on every steam railroad system is in freight, and it would hardly seem reasonable to suppose that any railway company would go to the enormous expense of equipping a road with electricity for the sake of operating a very small proportion of its traffic, namely, the passenger traffic. If that is the case it would then seem that we come back to the question of individual motor torque. We might stand the three trolley wires, but we have to get the equivalent of high acceleration in the motor. In other words, it is utterly out of the question to put an electric motor on every freight or coal car. It has to be a locomotive, and instead of starting a light mass and accelerating it rapidly, we have to start a heavy mass and accelerate it slowly, or we must have the torque and the tractive power concentrated in a locomotive for freight service. It would appear to me, therefore, that until the torque or acceleration capacity of the alternating current motor is greatly increased, the prospects are not very good for the general equipment of large railway systems with such motors.

MR. DE MURALT:—I will try to answer the questions in the order in which they were put. First and last, the acceleration. I admit that this is a very important question, but I do not quite

agree with Mr. Mailloux in saying that the alternating current motor is very much at a disadvantage in that respect. If in the Burgdorf-Thun railway we find only small accelerations, that is because no better are asked for by the officials of this road, but I believe that very much higher accelerations could easily be obtained with alternating current motors as well as with continuous current motors. It seems to me the question of acceleration is not really a question of the system; it is a question of putting enough power in motor-capacity in a car so as to get the required acceleration out of it. It does not really matter whether this power is in an alternating or in a continuous current motor.

Then in regard to the different points touched on by Mr. Stott, I do not know of any trouble that has arisen from poor contact made with that 10,000-volt line. I quite agree with him that if there should be any trouble it might be rather serious, and that a short-circuit might occur, but I think the pressure of the contact-making device with the wires can easily be so arranged, and particularly where the three wires are vertical, one above the other, as to cause no trouble whatever in this respect, especially as with 10,000 volts the current carried is of course very low.

As regards the distance between sub-stations, this of course depends to a great extent on the power required on the different parts of the road. In the particular case of the Burgdorf-Thun railway this power was rather large on account of the comparatively high grade. On a normal trunk line, 2½ per cent., I believe, is allowed rather a high grade, and this brings the sub-stations close together. In a case like the Manhattan Elevated or any road practically level, the sub-stations would be much further apart.

Regarding the lightning arrester on the sub-station, this is really one of the Siemens & Halske type, as the photograph shows, and I do not know that it has caused very much trouble. I have not investigated this question particularly, but I have seen lots of these lightning arresters installed, so I believe they must have given good service so far. I know though, that in a great many cases, lightning arresters of the Wurts type have also been installed, and as a matter of fact, even on the Burgdorf-Thun railway, some have been installed, probably to compare the two systems. None has been changed, so I think both must have given satisfaction.

As Mr. Hammer already remarked, the distance of the trolley wire from the ground is really not quite so important as President Steinmetz has pointed out, the bar contact device allowing also quite a variation in height. The contact device can be lowered so as barely to allow the wire to escape touching the car, and the contact will always be good.

The President also said that it would hardly suffice in a great many cases to have the car able to start on a 2½ per cent. grade. I quite agree with him, but the Burgdorf-Thun railway has no higher grade than this. Therefore, in this case it was not neces-

sary. If the road had higher grades, I think provision ought to be made so that cars could start under any circumstances, but I do not see any difficulty in this. It is merely a question of putting on enough motor capacity.

I do not know exactly the average headway actually allowed in the time-table of the Burgdorf-Thun railway, but it is easily found that as the stations are dimensioned so as to allow two trains running at the same time, the line can allow as many trains at least as there are sub-stations. So I think that is just about the minimum headway that can be allowed.

The drop in voltage on the roads I have studied is, I think, usually limited to about 6 to 10 per cent. on an average, but in the calculations, I think a maximum drop of 15 to perhaps 17 per cent. has been allowed. This seems rather high to me, and is perhaps not exactly safe.

I have not heard of any appreciable unbalancing being caused by the rail being used as the third conductor, although the frequency used is somewhat high. I think lower frequencies are more advisable in general, as with high frequencies the skin effect might play quite an important part.

Mr. C. E. L. Brown has said, I believe, that he chose the higher frequencies chiefly because of those roads being supplied by central stations furnishing power for the motor cars as well as for lighting installations. He could, therefore, not very well go much below the figures chosen. For a railroad alone, I think he would probably have used a lower frequency.

The contact devices used on the Burgdorf-Thun railway last very well during summer weather and ordinary climatic conditions. When there is sleet on the wires, however, they are used up rather quickly. I have no data at hand as to the mileage they can stand, but I am told that they usually last the whole summer season without changing, while in winter they are actually sometimes changed during one trip. It also depends a little upon how the motorman handles his car. It is, however, very interesting to remark that they are easily replaced, consisting merely of a tube pressed into the two outer parts of the steel frame, and the material of this tube, even when it is badly cut, is not lost and can easily be made into another tube, so there is perhaps not as much lost in the use of these contact devices as in the case of ordinary trolley wheels. These contact bars are usually made softer than the trolley wire, and this is probably the reason why I have not heard of the trolley wire being much worn. As a matter of fact, I have inquired into breakdowns on the Burgdorf-Thun railway and I was told they had been remarkably few, I think only two or three in the whole time.

The air-gap of the motors used is, if I am correctly informed, between $1\frac{1}{2}$ and 2 millimetres, which I think is just about what is standard for a normal stationary induction motor.

A MEMBER:—I would like to hear a word from the writer of the paper on the flatness of the top of the speed curve of which

our President spoke. What I refer to is the duplication of the old cable experience—not being able to make time.

MR. DE MURALT:—I am very glad you have touched on that point, because it gives me a chance to correct a current impression that only the alternating current system is at a disadvantage in that respect. Personally, I am not of that opinion, for the following reasons: If you want to make up time, what has been the cause? You have lost time either on account of the overloaded motor not having been able to run up to schedule time or else on account of the stops having been too long, because you have had too many passengers to handle. In both cases I do not think that the continuous current motor can make up time, because in both cases the load on the motor will be more than average, that is to say the speed will be really lower than average. Now the alternating current motor will under the same conditions also not be able to make up time, but in comparison with the continuous current motor, it will surely prevent losing more time. So I consider this question really an important one by showing an advantage rather for the alternating current motor than the continuous current motor.

MR. MAILLOUX:—I am very much surprised to hear Mr. de Muralt seriously express the opinion that there should not be any difference between the two forms of motors in the acceleration attainable. If that be true, then it must be possible to make induction motors that are capable of developing several times their running torque. Now I understand that while this may be theoretically possible, it is not industrially feasible. If we investigate the mathematical theory of acceleration, we readily see that the rate of acceleration possible depends exactly upon the amount of tractive effort or pull which the motor can exert over and above that which is necessary to overcome train resistance, grade resistance and all forces opposing the motion of the car; in other words, the difference between the total pull and the pull actually operative in overcoming train resistance of all kinds or what is called the net pull at that particular speed, represents the pull which forms the component of energy going into acceleration. Now, if we cannot obtain that net pull, I do not see how it is possible to get the acceleration. If we, for instance, can get a pull per ton, we will say, of 200 pounds, when 10 pounds is needed for overcoming train resistance, we will have 190 pounds of pull per ton available as the component of energy that goes into acceleration or as the force factor of the kinetic energy stored in the car. The rate of acceleration would be approximately 2.1 m.p.h. per second. If we can develop only 30 pounds, it is evident we have only 20 pounds available for acceleration, which would mean less than 0.25 m.p.h. per second. Let us suppose the running torque is more than that, because the train at full speed would have a higher train resistance. Let us suppose it is 30 pounds. In that case with our induction motor capable of developing, we will

say, 100 pounds per ton, we would have available 70 pounds per ton (or about 0.8 m.p.h. per second, whereas in the other case we would have 170 pounds (or about 1.9 m.p.h. per second.) That represents the difference between the two forms which is available for acceleration and which produces acceleration. I have investigated the matter very thoroughly, because I have had to deal with problems where I very seriously contemplated the use of alternating current motors, but I have found it impossible to expect of an inducton motor any acceleration exceeding 1¼ to 1½ miles per hour per second. I would be very glad indeed to know that it is possible to obtain it. On the other hand I know that with direct current motors there is no difficulty in going as high as 2½. I feel satisfied that the cases that I know of where I have seen alternating current motors used are all cases in which the acceleration is manifestly slow; so much so, as I have stated previously, as to be noticeable to American engineers accustomed to much higher accelerations. I would like very much to know of cases where a rate of acceleration exceeding 1½ miles per hour per second has been obtained with an induction motor. While it is theoretically possible to put on enough motor capacity, yet we all know that the limit is reached when we have so many horse power per ton of total weight. In other words, one might reach a point where the weight of motor necessary to produce the required acceleration was such that the train would only propel itself without any useful weight. Of course those are not industrial conditions, they are not commercial conditions, and yet they are precisely the conditions which have militated against the use of the induction motor in this country, as the President has very well pointed out. The conditions in this country are somewhat different. We have not so many cases where the distances are long and the stops few. On the contrary, the conditions all point to high speed, frequent stops, and conditions of a kind which absolutely preclude the use of the alternating current motor; and while I do not disbelieve in its possibilities, yet I certainly think that at the present time the field where it can be successfully used is extremely restricted. I am satisfied that it is not so much a question of the people waiting to see which railroad is going to use it first, as it is a question of the railroads waiting to see which other road is going to be fool enough to try it first. That is my opinion—in the present state of the art; I do not wish to state that things will always remain so, but I believe, as the President has very well pointed out, that in the present state of the art, and in ninety cases out of a hundred in this country, the direct current system is, on the whole, that which would meet most, if not all, the requirements; and if one is restricted to one system, and cannot have that which combines the advantages of two, the engineer will be almost sure to select the direct current system. In fact, he has no choice.

MR. DE MURALT:—I can perhaps agree with Mr. Mailloux that where extremely high accelerations are asked for, the alternating

current motor is, in the present state of the art, not quite as well suited as the continuous current motor. I would like to state, however, that accelerations of 1½ miles per hour per second have really been reached, within my knowledge, and I think even something above. I do not know anything of 2½. I do not think that has been reached, but I do think that it is really only a question of proportion between the running torque and the torque during acceleration. I do not exactly see why it should not be possible to design a motor powerful enough to give the acceleration wanted, as long as it does not exceed certain extreme limits, and I think this can be done as well with the alternating current motor as with the continuous current motor.

MR. MAILLOUX:—I would state that I think it can be done with some means of variable speed ratio, that is, some means in which the motor can be run up to full speed, and the car connected by means of some clutch device allowing the velocity ratio to be changed from a very high value to the lowest value required for the particular maximum car speed required. I very seriously doubt, however, if it will ever be done by the induction motor with any such methods of design, operation and control as have been under discussion this evening. In fact, I believe it is physically impossible to obtain an acceleration of 2 or 2½ miles per hour per second with such methods.

PRESIDENT STEINMETZ:..I do not agree that it is necessarily impossible to get accelerations of 2½, 3 or even more miles per hour per second from an induction motor. It can be done, but it is questionable whether it is commercially practicable. All this depends on the point of view. Now, I exclude first the case, occasionally met, where the conditions are so severe that the weight of the motor is a large percentage of the train weight; so that doubling the motor capacity of the train almost doubles the train weight and increases the dead weight so that twice the motor torque does not give you anything like twice the acceleration, and so an absolute limit of available acceleration is reached. But if we exclude these very severe conditions, it is true that by increasing the motor capacity we can get a very rapid acceleration. However, it depends on what consideration you base the motor capacity. As a rule, in designing a railway motor, capacity is based on the average load; that is, if we put say four 50-h.p. motors on a car, it means that the average load required of the motors will be such that four 50-h.p. motors will carry the car over the track. In other words, the amount of motors installed corresponds to the average load of the car, and the maximum acceleration which you can get corresponds to the maximum torque these motors can give. Now in the series motor, if you assume under average running condition, say 3 per cent. loss in the resistance of the motor, that means the maximum acceleration you can get is at least 33 times the average running torque, which is practically infinity, or beyond the slipping point of the wheels. That is with the continuous current motor you can get

any acceleration, up to the slipping point of the wheels. If you take the three-phase induction motor, and assume that the design of the motor capacity of the car corresponds to average running conditions, then the maximum torque of such motors will probably be three to four times the average torque and there you stop. To get twice this acceleration, you have to put in twice the motor capacity required for the average condition of running. Even if the train weight is not seriously increased, this merely means that the motors are underloaded, under average running conditions, the effect of which you know on the power factor and the efficiency. If I am not mistaken, induction motor propositions have been made where the concatenation of two motors in starting was used, and in running half the motors cut out, that is the locomotive was supplied with twice as much motor capacity as was necessary for running, merely to get the starting acceleration. This is not a very economical arrangement.

MR. MAILLOUX:—I only wish to add one word as a sort of clincher. It is this, the engineer who investigates this question has to consider one more question which perhaps is not included by the designer of motors or the manufacturer. That is the question of interest on the investment. When I said that these things are not practicable, I meant from the standpoint of one who makes an investment with a view of getting a financially satisfactory return on it. If we have a car so loaded that it cannot carry any useful freight, then it is evident that the cost per ton or the cost per passenger carried would have to be increased enormously to make that road pay, and the case is worse still if the cost of the equipment per ton carried be greatly increased at the same time. If all these things are considered, it will be found that the limitations are reached with the alternating current motor before attaining an acceleration such as was spoken of. That is what I meant.

MR. DE MURALT:—I am afraid that bringing in the question of investment and return on investment is putting the discussion on too general a basis, which, as I started to say, is not very good for discussion. I think we could easily find an extreme case to prove that a special road would be better off with alternating current, and we could also find another extreme case to prove that another special road would be better off with continuous current.

MR. MAILLOUX:—I did not mention a special case, except the special case of high acceleration. That is the only condition I stated, and I said that for those cases the alternating current motor was not suited. That at once states the condition; it limits it to a particular case.

PRESIDENT STEINMETZ:—If there are no further remarks, I call upon Mr. de Muralt to close the discussion.

MR. DE MURALT:—I think it is rather difficult for me to close this discussion, but I am glad to say that my paper brought out quite a number of points which I hope may have been interesting to you.

MR. MAILLOUX:—I would like to move a special vote of thanks to Mr. de Muralt for his paper. I think it is very gratifying to us here at this Annual Meeting to have one of our foreign members read a paper which is as interesting as Mr. de Muralt's paper has proved to be. While it may not be exactly according to our usual custom, I think we may make an exception in this case and extend a vote of thanks to our distinguished foreign member and guest. [The motion was seconded and carried.]

PRESIDENT STEINMETZ:—Mr. de Muralt, I do not quite acknowledge you a foreign member, because I know you have been living in this country for quite a number of years and also by other ties are very closely attached to the United States. In the name of the INSTITUTE I thank you very much for your very interesting paper.

THE SECRETARY:—There are 149 proxies represented to-night, and with the attendance in person this is more than the quorum required for a legal meeting by the Constitution, which is 10 per cent. of the membership.

[Adjourned.]

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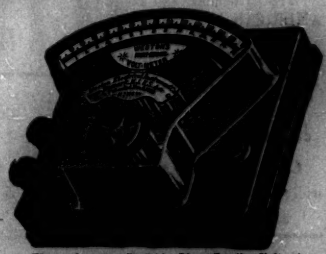

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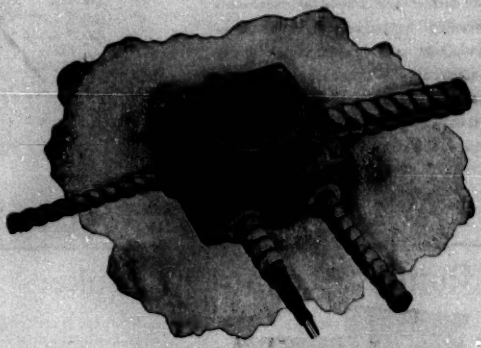

THE SPRAGUE ELECTRIC COMPANY

manufactures a larger variety of conduits and appliances than any other manufacturer in the world. The conduits made by this Company are designed for use in every climate and under all possible conditions, and represent the latest advance in the art of electric wiring. For each type of conduit there is a full line of junction boxes, fittings, tools etc. The best results are always obtained by using conduits and appliances manufactured by the

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ELECTRIC COMPANY

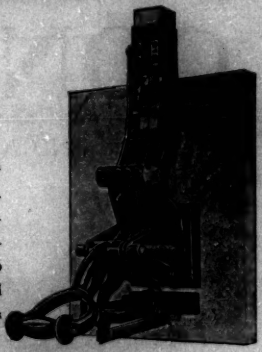
General Offices: 527-531 West 34th St., New York.

2713

Westinghouse

Type C

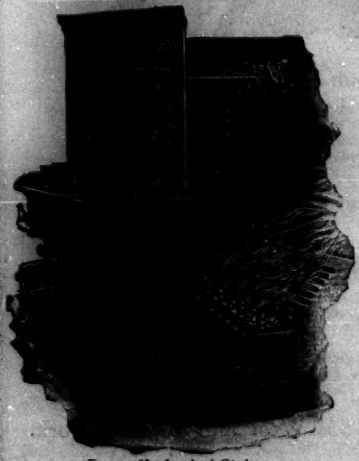
Circuit-Breakers



are built on the correct principle – an open arcwith carbon break above the metal parts. In opening, heavy copper shunts gradually withdraw current from copper to carbon contacts, and these resistance steps are so low that arcing cannot occur before the final break at the carbons. Under these conditions the *copper does not blister.*

Westinghouse Electric
and Manufacturing Co., Pittsburg, Pa.
Sales Offices in all Large Cities.

Roney Mechanical Stokers



Roney Mechanical Stoker.

form a part of the equipment of the majority of modern steam plants. Many hundred thousand horse-power of boilers of every type have been equipped as the result of the large economies in labor and fuel which this appliance effects.

Westinghouse, Church, Kerr & Co.,
ENGINEERS,
New York, Boston, Philadelphia, Pittsburg, Chicago, Detroit.

WHO ALSO CONTROL THE THE SALE OF THE PRODUCT OF

The Westinghouse Machine Co
Pittsburg—Chicago

BUILDERS OF
Westinghouse Junior, Standard, Compound and Corliss Steam Engines; Gas Engines, Steam Turbines.

2714

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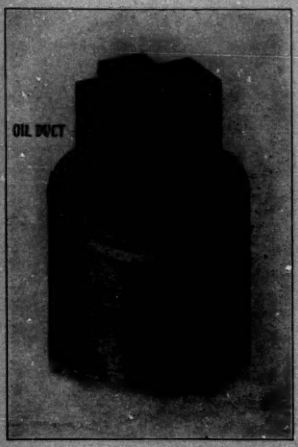
GENERAL ELECTRIC COMPANY'S

TYPE "H" TRANSFORMERS

CORE TYPE.



In type "H" Transformers, where it would not be economical to limit the thickness of coils to one and one-half inches, ducts for the circulation of cooling oil are provided between the windings and the iron core.

Unequal expansion of transformer windings, which always causes weakening of the mechanical and electrical strength of the insulations can be avoided only when temperatures throughout the windings are maintained perfectly uniform.

It is evident that this uniform heating can exist only when all parts of the windings are at a uniformly short distance from the cooling oil.

Type "H" Transformers —even of the largest size— never have any part of the copper more than one and one-half inches from the cooling oil. Therefore *temperatures and expansions are uniform* in all parts—a result even more desirable than low average temperature.

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